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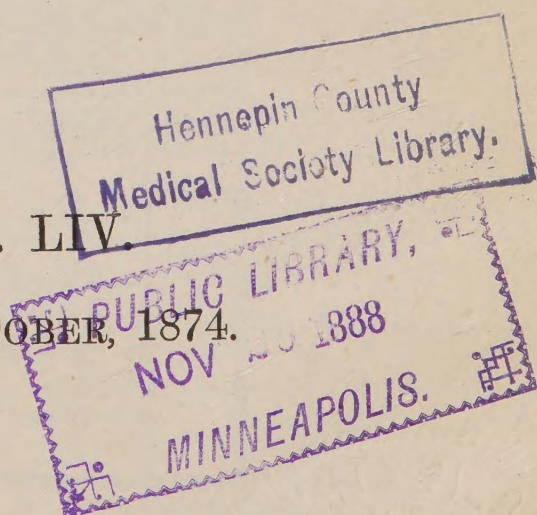


THE  
BRITISH AND FOREIGN  
MEDICO-CHIRURGICAL  
REVIEW

OR  
QUARTERLY JOURNAL  
OF  
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VOL. LIV.

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# CONTENTS OF NO. CVII.

## BRITISH AND FOREIGN

## MEDICO-CHIRURGICAL REVIEW.

JULY, 1874.

### Analytical and Critical Reviews.

|   | PAGE |
|---|------|
| REV. I.—1. The Antiseptic System: a Treatise on Carbolic Acid and its Compounds; with Enquiries into the Germ Theories of Fermentation, Putrefaction, and Infection; and the Practical Applications of Antiseptics, especially in Medicine and Surgery. By ERNEST SANSOM, M.D. Lond. 1871 | 1    |
| 2. An Inquiry into the Theory and Practice of Antiseptic Surgery. By JAMES CUMMING, M.D., late Resident Surgeon, Clinical Wards, Royal Infirmary, Edinburgh. 1872   | ib.  |
| 3. Observations on some of the more Recent Methods of treating Wounds. By EDWARD LUND, F.R.C.S., Surgeon to the Manchester Royal Infirmary  | ib.  |
| 4. The Isolation and Treatment of Wounds. Read before the Surgical Section of the British Medical Association at King's College, August, 1873. By GEORGE N. CALLENDER, F.R.S.   | ib.  |
| 5. The After-treatment of Large Amputations. By ROBERT HAMILTON, F.R.C.S., Surgeon to the Royal Southern Hospital, Liverpool. 1874  | ib.  |
| 6. The Use of Earth in Surgery as a Topical Application. By ADDINELL HEWSON, M.D., one of the Attending Surgeons to the Pennsylvania Hospital. 1872   | ib.  |
| REV. II.—1. LUNIER, M. L.—Influence des Evénements de 1870-1871 sur le Mouvement de l'Aliénation Mentale en France. 'Annales Médico-psychologiques,' Septembre, 1872, Mars, Mai, Juillet, Septembre, November, 1873   | 17   |
| 2. LABORDE, J. V.—Les Hommes et les Actes de l'Insurrection de Paris. Paris, 1872   | ib.  |
| 3. COLLINEAU, M. le Dr.—Les Commotions Politiques dans leurs Rapports avec l'Aliénation. Paris, 1873  | ib.  |
| 4. CLÈRE, M. JULES.—Les Hommes de la Commune: Biographie complète de tous ses Membres. Deuxième Edition. Paris, 1871  | ib.  |
| REV. III.—1. Quarantaines. Par M. le Dr. LEON COLIN, Médecin principal de l'Armée, Professeur d'Epidémiologie à l'école du Val-de-Grâce. Extrait du 'Dictionnaire Encyclopédique des Sciences Médicales,' Mai, 1873. Paris  | 30   |
| 2. Quarantine on the Southern and Gulf Coasts of the United States. By HARVEY E. BROWN, Assistant-Surgeon, United States Army. New York, 1873   | ib.  |
| 3. Abstract of Regulations in force in Foreign Countries respecting Quarantine. (Parliamentary Paper), May, 1860  | ib.  |



|   | PAGE |
|---|------|
| 4. Abstracts of Returns of Information on the Laws of Quarantine. (Parliamentary Paper), August, 1860 . . . . .   | 30   |
| 5. Papers relating to Quarantine, communicated to the Board of Trade. (Parliamentary Paper), August, 1861 . . . . .   | ib.  |
| REV. IV.—Rabies and Hydrophobia; their History, Nature, Causes, Symptoms, and Prevention. By GEORGE FLEMING, F.R.G.S., M.A.I.; President of the Central Veterinary Medical Society; Veterinary Surgeon, Royal Engineers. Pp. 405. London, 1872 . . . . .              | 49   |
| REV. V.—The Medical and Surgical History of the War of the Rebellion, 1861-65. Prepared under the direction of Surgeon-General J. K. BARNES, U.S.A. Washington, 1870 . . . . .  | 65   |
| REV. VI.—1. Anatomische Beschreibung eines Mikrocephalen 8 jährigen Mädchens. Von Dr. Th. L. W. v. BISCHOFF . . . . .   | 81   |
| Anatomical Description of a Microcephalic Brain of a Female Child. By Dr. Th. L. W. v. BISCHOFF . . . . .   | ib.  |
| 2. On the Brain of a Bushwoman; and on the Brains of Two Idiots of European Descent. By JOHN MARSHALL, F.R.S. . . . .   | ib.  |
| REV. VII.—1. Cerebral Hæmorrhage and Apoplexy. By J. HUGHLINGS JACKSON, M.D., F.R.C.P. ('Reynolds's System of Medicine,' vol. ii, 2nd edition) . . . . .  | 87   |
| 2. A Treatise on Apoplexy, &c. By JOHN H. LIDELL, A.M., M.D. . . . .  | ib.  |
| 3. Congestion of the Brain. By J. RUSSELL REYNOLDS, M.D., F.R.S., and H. CHARLTON BASTIAN, M.D., F.R.S. (Reynolds's System of Medicine,' vol. ii, 2nd edition) . . . . .  | ib.  |
| 4. Encéphale.—Congestion. Par JACCOUD et HALLOPEAU. ('Dictionnaire de Médecine, &c.,' tome xiii) . . . . .  | ib.  |
| 5. Hyperæmia of the Brain and its Membranes. By Dr. F. VON NIEMEYER, M.D. Translated by Drs. HUMPHREYS and HACKLEY . . . . .  | ib.  |
| REV. VIII.—Reports of the Inspectors of Factories for the Year ending October, 1873 . . . . .   | 107  |
| REV. IX.—A Manual of Psychological Medicine, containing the Lunacy Laws, the Nosology, Etiology, Diagnosis, Treatment, &c., of Insanity, with an Appendix of Cases. By J. C. BUCKNILL, M.D., F.R.S., and DANIEL HACK TUKE, M.D. Third Edition. London, 1874 . . . . . | 115  |
| REV. X.—1. Skin-Grafting. By JOHN WOODMAN, F.R.C.S., Medical Officer to the City Workhouse, Exeter. Second Edition. 1873, pp. 31 . . . . .  | 121  |
| 2. Sul Trapianto Cutaneo. Parole del DOTT. AGGIO CASELLI, Chirurgo primario nell' ospedale di S. Maria Nuova in Reggio-Emilia. Bologna. 1873, pp. 10 . . . . .  | ib.  |
| REV. XI.—De l'Inflammation, et de la Circulation. Par le Prof. M. SCHIFF, Traduction de l'Italien, par le Dr. R. GUICHARD DE CHOISITY. Paris. 1873, pp. 96 . . . . .  | 128  |
| REV. XII.—The Conservation of Energy, an Elementary Treatise on Energy and its Laws. By BALFOUR STEWART, M.A., LL.D., F.R.S., Professor of Natural Philosophy at the Owen's College, Manchester . . . . .   | 134  |
| REV. XIII.—Modern French Surgery. Part II (continued). . . . .  | 138  |

### Bibliographical Record.

|  |     |
|--|-----|
| ART. I.—Memorials of the Life of James Syme, Professor of Clinical Surgery in the University of Edinburgh, &c. By ROBERT PATERSON, M.D., &c. Edinburgh, 1874 . . . . . | 147 |
| ART. II.—The Student's Guide to Zoology: a Manual of the Principles of Zoological Science. By ANDREW WILSON. 1874 . . . . .  | 150 |
| ART. III.—Introduction to the Study of Organic Chemistry. By H. E. ARMSTRONG. Pp. x and 349. London, 1874 . . . . .  | 152 |



|   | PAGE |
|---|------|
| ART. IV.—Milk Analysis: a Practical Treatise. By J. A. WANKLYN. Pp. vi and 70. London, 1874 . . . . .   | 154  |
| ART. V.—The Birth of Chemistry. By G. F. RODWELL. Pp. xii and 135. London, 1874 . . . . .   | 156  |
| ART. VI.—A Manual of the Chemistry of the Carbon-Compounds. By C. SCHORLEMMER, F.R.S. Pp. xii and 512. London, 1874 . . . . .   | ib.  |
| ART. VII.—The New Chemistry. By J. P. COOKE, of Harvard University. Pp. 326. London, 1874 . . . . .   | 157  |
| ART. VIII.—The Laboratory Guide; a Manual of Practical Chemistry, specially arranged for Agricultural Students. By A. H. CHURCH, Professor of Chemistry in the Agricultural College, Cirencester. Third edition, enlarged and revised. London, 1874 . . . . . | 158  |
| ART. IX.—Qualitative Chemical Analysis and Laboratory Practice. By J. E. THORPE and M. M. P. MUIR. Pp. xv, and 238. London, 1874 . . . . .  | 159  |
| ART. X.—Lectures on the Diseases of Infancy and Childhood. By CHARLES WEST, M.D., &c. Sixth edition, revised and enlarged. London, 1874 . . . . .   | ib.  |
| ART. XI.—Manual of Lunacy: a Handbook relating to the Legal Care and Treatment of the Insane, &c. By LYTTLETON S. WINSLOW, M.B., &c. With a preface by FORBES WINSLOW, M.D., D.C.L., &c. London, 1874 . . . . .   | 162  |
| ART. XII.—The Medical Adviser in Life Assurance. By EDWARD H. SIEVEKING, M.D., &c. London, 1874 . . . . .   | 163  |
| ART. XIII.—The Journal of Anatomy and Physiology. Conducted by Prof. HUMPHRY and Prof. TURNER. Second Series, No. XIII. 1873 . . . . .  | 166  |
| ART. XIV.—The Anatomist's Vade-Mecum, a System of Human Anatomy. By ERASMUS WILSON, F.R.S. Edited by G. BUCHANAN, M.D., assisted by H. E. CLARK. Ninth edition. London, 1873. Pp. 743 . . . . .   | 167  |
| ART. XV.—The Bible and the Doctrine of Evolution, being a complete Synthesis of their Truth, and giving a sure Scientific Basis for the Doctrines of Scripture. By W. WOODS SMYTH. London, 1873 . . . . .   | ib.  |
| ART. XVI.—Sketch of Cannes and its Climate. By TH. de VALECOURT, M.D., &c. Second edition, enlarged. London, 1873 . . . . .   | 168  |
| ART. XVII.—Proceedings of the Pathological Society of Philadelphia, vol. iii . . . . .  | 169  |
| ART. XVIII.—Proceedings of the Dublin Obstetrical Society for Session 1872-73 . . . . .   | ib.  |
| ART. XIX.—Transactions of the Pathological Society of London, vol. xxiv. London, 1873 . . . . .   | 170  |
| ART. XX.—Transactions of the Obstetrical Society of London (vol. xv) for the Year 1873. London, 1874 . . . . .  | 171  |
| ART. XXI.—Nouveau Dictionnaire de Médecine et de Chirurgie pratiques. Tome xviii. Hyd.—Infl. Avec 43 figures intercalées dans le texte. Paris, 1874 . . . . .   | 172  |
| ART. XXII.—A Practical Treatise on the Diseases of Children. By J. FORSYTH MEIGS, M.D., and WILLIAM PEPPER, M.D. Fifth edition, revised and enlarged. London, 1874 . . . . .  | ib.  |
| ART. XXIII.—The Domestic Management of Children. By T. M. BRAIDWOOD, M.D. London, 1874 . . . . .  | 174  |
| ART. XXIV.—On the Temperature of the Body as a means of Diagnosis and Prognosis in Phthisis. By SIDNEY RINGER, M.D. Second edition. London, 1873 . . . . .  | ib.  |
| ART. XXV.—Aide-Mémoire de Pharmacie: Vade-Mecum du Pharmacien. Par EUSÈBE FERRAUD. Avec 184 figures. Paris, 1873 . . . . .  | 175  |
| ART. XXVI.—A Manual of the Operations of Surgery, for the use of Senior Students, House-Surgeons, and Junior Practitioners. Illustrated. By JOSEPH BELL, F.R.C.S. Edin. &c. Third edition, revised and enlarged. Edinburgh, 1874 . . . . .                    | 176  |
| ART. XXVII.—Du Traitement des Rétrécissements de l'urèthra par la dilatation progressive. Par T. B. CURTIS (Boston, U.S.A.), M.D. Paris.  |      |

|  | PAGE |
|--|------|
| Travail couronné par la commission du prix Civiale pour l'année 1872.<br>Paris, 1873   | 176  |
| ART. XXVIII.—Diseases of the Urinary Organs; including Stricture of<br>the Urethra, Affections of the Prostate, and Stone in the Bladder.<br>By JOHN W. S. GOULAY, M.D., late Professor of Surgery in the<br>University of New York. New York, 1873. Pp. 368 | 177  |
| ART. XXIX.—Diseases of the Skin: in Twenty-four Letters on the Prin-<br>ciples and Practice of Cutaneous Medicine. By HENRY EVANS<br>CANTY, Surgeon to the Liverpool Dispensary for Diseases of the Skin.<br>Pp. 365. London, 1874                           | 179  |
| ART. XXX.—Traité des Sections Nerveuses. Par E. LÉTIÉVANT, Chirur-<br>gien en chef esigné de l'Hôtel Dieu de Lyon, &c. Paris, 1873   | 181  |
| ART. XXXI.—Surgical Inquiries. By FURNEAUX JORDAN, F.R.C.S. Lon-<br>don. 1873  | 184  |

### Original Communications.

|  |     |
|--|-----|
| ART. I.—Miliary Sclerosis—its Pathological Significance. By W. B.<br>KESTEVEN, F.R.C.S.          | 187 |
| ART. II.—Résumé of the Report on Epidemics of Cholera in France, in 1832,<br>1849, 1855 and 1865 | 193 |

### Chronicle of Medical Science.

(CHIEFLY FOREIGN AND CONTEMPORARY.)

|  |     |
|--|-----|
| Report on Scandinavian Medicine. By J. W. MOORE, M.D., M.Ch. Dubl.,<br>F.K.Q.C.P.  | 210 |
| Report on Surgery. By HENRY A. REEVES, F.R.C.S.E.  | 230 |
| Report on Toxicology, Forensic Medicine, and Hygiene. By BENJAMIN W.<br>RICHARDSON, M.D., F.R.S.   | 242 |
| Report on Physiology. By HENRY POWER, F.R.C.S., M.B. Lond., Senior<br>Ophthalmic Surgeon to, and Lecturer on Ophthalmic Diseases at, St.<br>Bartholomew's Hospital | 254 |
| BOOKS RECEIVED FOR REVIEW  | 264 |

# CONTENTS OF NO. CVIII.

## BRITISH AND FOREIGN

### MEDICO-CHIRURGICAL REVIEW.

OCTOBER, 1874.

#### Analytical and Critical Reviews.

|   | PAGE |
|---|------|
| REV. I.—1. Principles of Mental Physiology : with their Applications to the Training and Discipline of the Mind and the Study of its Morbid Conditions. By WILLIAM B. CARPENTER, M.D., &c. &c. London, 1874 | 269  |
| 2. Body and Mind : an Inquiry into their Connexion and Mutual Influence ; specially in reference to Mental Disorders, &c. An enlarged and revised Edition. By HENRY MAUDSLEY, M.D., &c. London, 1873        | ib.  |
| 3. Responsibility in Mental Disease. By HENRY MAUDSLEY, M.D., &c. 1874  | ib.  |
| 4. On certain Disorders and Defects of Organic Memory. By THOMAS LAYCOCK, M.D., &c. 'Edinburgh Medical Journal,' April, 1874  | ib.  |
| 5. The Physical Basis of Mental Life. By R. R. NOEL. 8vo. London, 1873. Pp. 74  | ib.  |
| 6. Mind and Body : the Theories and Principles of their Relation. By ALEX. BAIN, LL.D. 8vo. London, 1874. Pp. 196   | ib.  |
| 7. Problems of Life and Mind. By GEORGE HENRY LEWES. First Series : The Formation of a Creed. 8vo. London, 1874. Pp. 472  | ib.  |
| 8. A Treatise on the Nature of Man regarded as Triune : with an Outline of the Philosophy of Life. By THOMAS BEST WOODWARD. 8vo. London, 1874. Pp. 277  | ib.  |
| 9. What am I ? a Popular Introduction to Mental Philosophy and Psychology. By EDWARD W. COX, Serjeant-at-Law. 8vo. London, Vol. I, 1873 ; Vol. II, 1874   | ib.  |
| REV. II.—1. Epidemiology ; or, the Remote Cause of Epidemic Diseases in the Animal and in the Vegetable Creation. By JOHN PARKIN, M.D., F.R.C.S. London, 1873. Part I                                       | 292  |
| 2. The Pathology and Treatment of Smallpox. By ROBERT H. BAKEWELL, M.D. London, 1872  | ib.  |
| 3. Smallpox : the Predisposing Conditions and their Preventives, with a Scientific Exposition of Vaccination. By Dr. CARL BOTH. Boston, 1872  | ib.  |
| 4. How to Prevent Smallpox : being Plain Facts on Vaccination, and Hints on Sanitary Precautions. By MORDEY DOUGLAS, M.R.C.S. London, 1871  | ib.  |
| 5. Notes on Smallpox and its Treatment. By W. GAYTON. London, 1873  | ib.  |
| 6. Report on the Smallpox Epidemic, 1871-73, as Observed in Cork Street Fever Hospital. By THOMAS WRIGLEY GRIMSHAW, M.D. Dublin, 1873   | ib.  |
| 7. La Variole au point de vue épidémiologique et prophylactique. Par LÉON COLIN. Paris, 1873  | ib.  |
| REV. III.—1. Traité de l'Alimentation dans ses Rapports avec la Physiologie, la Pathologie, et la Thérapeutique. Par le Docteur JULES CYR. Paris, 1869  | 306  |
| 2. Foods. By EDWARD SMITH, M.D., LL.B., F.R.S. London, 1873   | ib.  |
| 3. A Treatise on Food and Dietetics, Physiologically and Therapeutically Considered. By F. W. PAVY, M.D., F.R.S. London, 1874   | ib.  |
| 4. 35th & 36th Vict., ch. 74 (commonly known as "The Adulteration Act, 1872")   | ib.  |



|  | PAGE |
|--|------|
| REV. IV.—The Puerperal Diseases. Clinical Lectures, delivered at Bellevue Hospital, New York. By FORDYCE BARKER, M.D. London, 1874   | 318  |
| REV. V.—1. <i>Traité théorique et pratique d'Hydrothérapie.</i> Par le Dr. BENI-BARDE. 1874  | 328  |
| 2. <i>Nouveau Dictionnaire de Médecine et de Chirurgie pratiques.</i> Tome xviii, 1874. Art. "L'Hydrothérapie"   | ib.  |
| 3. <i>Hydropathy as applied to Acute Diseases.</i> By T. R. ARMITAGE, M.B. London, 1852  | ib.  |
| 4. <i>Deutsches Archiv für Klinische Medecin.</i> Vol. I. LIEBERMEISTER, "Ueber Febrilen Temperatursteigerungen"   | ib.  |
| 5. Id., Vol. IV. LIEBERMEISTER, "Bericht über die Resultate der Behandlung des Abdominaltyphus im Spital zu Basel"   | ib.  |
| 6. Id., Vol. VIII. G. MERKEL, "Die Resultate der Kaltwasserbehandlung des Typhus im städtischen Krankenhause zu Nürnberg während des Jahres 1869"  | ib.  |
| 7. W. LEUBE, "Mittheilungen über die Typhusepidemie in der Festung Ulm in Winter 1870-71, und über ihre Behandlung im dortigen Barakenspitale"   | ib.  |
| 8. BÖHM und MICHEL, "Beobachtungen über die Kaltwasserbehandlung des Abdominal Typhus in Kriege"   | ib.  |
| 9. Id., Vol. IX. SCHOLZ, "Bericht über die Resultate der Kaltwasserbehandlung des Unterleibstyphus in der Krankenanstalt zu Bremen"  | ib.  |
| 10. F. RIEGEL, "Ueber die Resultate der Kaltwasserbehandlung der Unterleibstyphus in K. Julius Hospitale zu Würzburg im Jahre, 1870 und '71"   | ib.  |
| 11. 'Prager Vierteljahrschrift.' Vol. 113, 1872. GÖLZ, "Beobachtungen und Erfahrungen über die Anwendung des Kaltenwassers beim Typhus"  | ib.  |
| REV. VI.— <i>De la Régénération des Organes et des Tissus.</i> Par J. N. DEMARQUAY, Chirurgien de la Maison Municipale de Santé, &c., avec quatre planches. Paris, 1874. Pp. 328, large 8vo  | 343  |
| REV. VII.— <i>Contributions to Pathology and Surgery.</i> By CÆSAR H. HAWKINS, F.R.S., &c. London, 1874  | 348  |
| REV. VIII.— <i>Five Years' Surgical Work in the Manchester Royal Infirmary.</i> By EDWARD LUND, F.R.C.S., one of the Surgeons to the Hospital, and Member of the Senate of Owen's College, Manchester. Reprinted from the 'Liverpool and Manchester Medical and Surgical Reports, 1874.' Manchester, 1874  | 352  |
| REV. IX.—1. <i>Diseases of the Urinary Organs, including Stricture of the Urethra, Affections of the Prostate, and Stone in the Bladder.</i> By JOHN W. S. GOULEY, M.D., late Professor of Clinical Surgery and Genito-urinary Diseases in the Medical Department of the University of the City of New York, Surgeon to Bellevue Hospital, &c. With 103 wood engravings. New York and London, 1873. Pp. 368  | 357  |
| 2. <i>A Practical Treatise on the Surgical Diseases of the Genito-urinary Organs, including Syphilis, designed as a Manual for Students and Practitioners, with Engravings and Cases.</i> By W. H. VAN BUREN, A.M., M.D., Professor of the Principles of Surgery, with Diseases of the Genito-urinary System and Clinical Surgery, in Bellevue Hospital Medical College, &c.; and E. L. KEYES, A.M., M.D., Professor of Dermatology in Bellevue Hospital Medical College, Surgeon to the Charity Hospital, Venereal Division, &c. London, 1874. Pp. 672  | ib.  |
| 3. <i>Traité pratique des Maladies des Voies Urinaires.</i> Par Sir H. THOMPSON, F.R.C.S., Professeur de Clinique Chirurgicale et Chirurgien à University College Hospital; Chirurgien Extraordinaire de S.M. le Roi des Belges, &c., Traduit avec l'Autorisation de l'auteur et Annotée par EDOUARD MARTIN, EDOUARD LABARRAQUE, et VICTOR CAMPENON, Internes des Hôpitaux de Paris, Membres de la Société Anatomique; précédé des Leçons cliniques sur les Maladies des Voies Urinaires, Professées à University College Hospital, Traduites et Annotées par le Docteurs JUDE HUE et F. GIGNOUX. Avec 280 figures. Paris, 1874. Pp. 775 | ib.  |

|  | PAGE |
|--|------|
| 4. Leçons Cliniques sur les Maladies des Voies Urinaires, professées à University College Hospital de Londres. Par Sir HENRY THOMPSON, Chirurgien Extraordinaire de S.M. le Roi des Belges, &c.; Traduites, Annotées et Augmentées d'une Introduction Anatomique, par les Docteurs JUDE HUE, Ex-chirurgien en Chef de l'Ambulance Internationale Rouennaise, &c., et F. GIGNOUX, Ancien Interne de l'Hôpital Saint Éloi, &c.; Ouvrage contenant 40 gravures sur bois et 3 Leçons de plus que la Troisième et Dernière Édition Anglaise. Paris, 1874. Pp. 530 . . . . . | 357  |
| 5. Du Traitement des Rétrécissements de l'Urethre par la Dilatation progressive. Par T. B. CURTIS, Docteur en Médecine de la Faculté de Paris, &c.; Travail couronné par le Commission du Prix Civiale, 1872. Paris, 1873. . . . .   | ib.  |
| 6. Cases of Stricture. By W. F. TEEVAN. 'Transactions of the Clinical Society of London,' vols. iv, v, and vi, 1871, '72, '73 . . . . .  | ib.  |
| REV. X.—1. A Treatise on the Continued Fevers of Great Britain. By CHARLES MURCHISON, M.D., LL.D., F.R.S., &c. Second Edition. London, 1873. Pp. 729 . . . . .   | 367  |
| 2. Typhoid Fever; its Nature, Mode of Spreading, and Prevention. By WILLIAM BUDD, M.D., F.R.S. London, 1873. Pp. 193 . . . . .   | ib.  |
| 3. Lectures on Fever. By WILLIAM STOKES, M.D., D.C.L. Oxon., F.R.S. London, 1874. Pp. 459 . . . . .  | ib.  |

### Bibliographical Record.

|  |     |
|--|-----|
| ART. I.—Life on the Gold Coast. By CHARLES ALEXANDER GORDON, M.D., C.B., Deputy Surgeon-General. London, 1874 . . . . .  | 392 |
| ART. II.—1. A Practical Treatise on the Diseases of the Ear, including the Anatomy of the Organ. By D. B. ST. JOHN ROOSA, M.A., M.D., Professor of Diseases of the Eye and Ear in the University of the City of New York . . . . .   | 394 |
| 2. Traité Théorique et Pratique des Maladies de l'Oreille et des Organes de l'Audition. Par le Docteur J. P. BONNAFONT. Deuxième Édition, revue et augmentée . . . . .   | ib. |
| 3. The Questions of Aural Surgery. By JAMES HINTON, Aural Surgeon to Guy's Hospital . . . . .  | ib. |
| 4. Lectures on Aural Catarrh; or, the Commonest Forms of Deafness and their Cure. By PETER ALLEN, M.D., F.R.C.S. Edin., Aural Surgeon to St. Mary's Hospital . . . . .   | ib. |
| ART. III.—Lectures on Practical Pharmacy. By BARNARD S. PROCTOR. London, 1873 . . . . .  | 400 |
| ART. IV.—Surgical Emergencies; together with the Emergencies attendant on Parturition, and the Treatment of Poisoning; a Manual for the use of General Practitioners. By WILLIAM PAUL SWAIN, F.R.C.S., Surgeon to the Royal Albert Hospital, Devonport. London, 1874 . . . . . | 402 |
| ART. V.—Lessons in Laryngoscopy, including Rhinoscopy and the Diagnosis and Treatment of Diseases of the Throat. By PROSSER JAMES, M.D. London, 1873. Pp. 176 . . . . .  | 405 |
| ART. VI.—Practical Medicine, with a Sketch of Pathology and Therapeutics; being the Fourth Edition of 'Meade's Manual for Students.' By ALEXANDER SILVER, M.D., &c. London, 1874 . . . . .   | 406 |
| ART. VII.—The Science and Practice of Medicine in relation to Mind, the Pathology of Nerve Centres, and the Jurisprudence of Insanity; being a Course of Lectures delivered in Guy's Hospital. By J. THOMPSON DICKSON, M.A., M.B. Cantab. London, 1874 . . . . .               | 407 |
| ART. VIII.—Legal Responsibility in Old Age; based on Researches into the Relations of Age to Work. By GEORGE M. BEARD, A.M., M.D. (Read before the Medico-Legal Society of the City of New York, March, 1873.) New York, 1874 . . . . .  | 411 |
| ART. IX.—The Psychology of Scepticism and Phenomenalism. By JAMES ANDREWS. Glasgow, 1874 . . . . .   | 412 |

|  | PAGE |
|--|------|
| ART. X.—Ueber Progressive Muskelatrophie, über Wahre und Falsche Muskelhypertrophie. Von Dr. N. FRIEDREICH, Professor der Medecin in Heidelberg. 4to, with two plates. Berlin, 1873.   | 414  |
| ART. XI.—La Folie Héréditaire (Hereditary Insanity). Par le Dr. LEGRAND DE SAULLE. Pp. 74. Paris, 1873   | ib.  |
| ART. XII.—Le Délire des Persecutions. Par le Dr. LEGRAND DE SAULLE. 8vo, pp. 524. Paris, 1873  | 415  |
| ART. XIII.—Elephantiasis Græcorum, or True Leprosy. By ROBERT LIVEING, A.M. and M.D. Cantab., F.R.C.P. Lond.; Physician to the Middlesex Hospital. The 'Gulstonian Lectures for 1873,' revised and enlarged. London, 1873. 8vo, pp. 150  | 416  |
| ART. XIV.—Longevity; the Means of Prolonging Life after Middle Age. By JOHN GARDNER, M.D. Pp. 168. London, 1874  | 419  |
| ART. XV.—On the Operative Surgery of the Foot and Ankle-Joint. By HENRY HANCOCK, late President of the Roy. Col. of Surgeons, &c. 1873   | 420  |
| ART. XVI.—New Editions of 'Thompson's Conspectus, adapted to the British Pharmacopœia,' edited by Dr. BIRKETT; of the 'Syllabus of Materia Medica,' by Drs. ALEX. HARVEY and A. D. DAVIDSON; of Garrod's Essentials of 'Materia Medica,' edited by Dr. BAXTER; and of 'Pereira's Materia Medica,' abridged by Professors BENTLEY and REDWOOD | 424  |
| ART. XVII.—Harvey and his Times; the Harveian Oration for 1874. By CHARLES WEST, M.D., &c. London  | ib.  |
| ART. XVIII.—An Epitome of Therapeutics, being a Comprehensive Summary of the Treatment of Disease as recommended by the Leading British, American, and Continental Physicians. By W. DOMETT STONE, M.D. London, 1874   | ib.  |
| ART. XIX.—A Lecture on the Diagnosis of Disease of the Heart. By GEORGE W. BALFOUR, M.D. Edinburgh, 1874   | 425  |
| ART. XX.—Hints for Health; being Two Lectures on the Influence of Air, Water, Food and Wine, on the System. By J. STOCKER, M.D. London, 1874   | 426  |
| ART. XXI.—Autobiographical Recollections of the Medical Profession. By J. F. CLARKE, M.R.C.S. London, 1874   | ib.  |
| ART. XXII.—The Tongue not essential to Speech, with Illustrations of the Power of Speech in the African Confessors. By the Hon. EDWARD TWISLETON. London, 1873. Pp. 232  | ib.  |

### Original Communications.

|   |     |
|---|-----|
| I.—On Granular Disease of the Conjunctiva and Contagious Ophthalmia. By EDWARD NETTLESHIP, F.R.C.S., Surgeon to the South London Ophthalmic Hospital; late Clinical Assistant and Curator of the Museum at Moorfields Ophthalmic Hospital | 429 |
| II.—The Transmissibility of Tuberculosis. By GEORGE FLEMING, M.R.C.V.S., F.R.G.S., Royal Engineers  | 461 |

### Chronicle of Medical Science.

(CHIEFLY FOREIGN AND CONTEMPORARY.)

|  |     |
|--|-----|
| Report on Pathology and the Practice of Medicine. By JOHN T. ARLIDGE, M.D., A.B. Lond., F.R.C.P. Lond., &c.              | 487 |
| Report on Materia Medica and Therapeutics. By ROBERT HUNTER SEMPLE, M.D., Fellow of the Royal College of Physicians, &c. | 500 |
| Report on Obstetrics, Gynæcology, and Pædiatrics. By ALFRED WILTSHIRE, M.D., M.R.C.P. Lond., &c.                         | 513 |
| BOOKS RECEIVED FOR REVIEW  | 526 |

INDEX, CONTENTS, &c.



THE  
BRITISH AND FOREIGN  
MEDICO-CHIRURGICAL REVIEW.

JULY, 1874.

Analytical and Critical Reviews.

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I.—Antiseptic Surgery.<sup>1</sup>

NOTHING in medical art since the days of old Hippocrates better illustrates the profound truth of the reflection which troubled him about the fallaciousness of experience and the difficulty of judgment, than the differences of opinion which at present exist in authoritative quarters about the surgical treatment of open wounds. When that other shrewd medical worthy—Sir Kenelm Digby—recommended that the wound should be simply kept clean and dressed with water, while the sympathetic medicaments were applied to the weapon which inflicted it, he established the first judicious compromise between the real unfettered powers of nature and the pompous pretensions of art in the process of healing. His sympathetic doctrine formed the necessary pretext and apology for his expectant treatment. Twenty years ago this simple plan of dressing wounds had, in this country, by the influential teaching of

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<sup>1</sup> 1. *The Antiseptic System: a Treatise on Carbolic Acid and its Compounds; with Enquiries into the Germ Theories of Fermentation, Putrefaction, and Infection; and the Practical Applications of Antiseptics, especially in Medicine and Surgery.* By ERNEST SANSOM, M.D. Lond. 1871.

2. *An Inquiry into the Theory and Practice of Antiseptic Surgery.* By JAMES CUMMING, M.D., late Resident Surgeon, Clinical Wards, Royal Infirmary, Edinburgh. 1872.

3. *Observations on some of the more Recent Methods of Treating Wounds.* By EDWARD LUND, F.R.C.S., Surgeon to the Manchester Royal Infirmary.

4. *The Isolation and Treatment of Wounds.* Read before the Surgical Section of the British Medical Association at King's College, August, 1873. By GEORGE N. CALLENDER, F.R.S.

5. *The After-treatment of Large Amputations.* By ROBERT HAMILTON, F.R.C.S., Surgeon to the Royal Southern Hospital, Liverpool. 1874.

6. *The Use of Earth in Surgery as a Topical Application.* By ADDINELL HEWSON, M.D., one of the Attending Surgeons to the Pennsylvania Hospital. 1872.

Syme and Liston, become the rule of treatment, with frequent recurrence to the supposed soothing effect of emollient poultices so appreciatingly applied by Abernethy. To the present day even this pultaceous predilection still adheres to some with sticky pertinacity. Among the indications of the general reaction against the old system of complicated pharmacy, the simple plan of cold and warm water dressing possessed a powerful charm to the practical chirurgical mind, always more sceptical of medicaments than that of his more purely medical brother, whose mode of working, more learned and subtle, is certainly more obscure. The more the ideas of water and air were associated with those of cleanliness and purity, the more the practice grew into professional and general favour. But as the ever growing, illuminating, and defining powers of the test-tube and microscope were turned upon these ideals of purity, they became, like so many other ideals, very much discredited. It was found in the search into the causes of epidemic diseases, especially of cholera and typhus, that the most sparkling and translucent fluids might become the most treacherous means of introducing into the human body poisons deadly as that of the Indian serpent. Although one of the most eminent in his own professional walk seems still to cling fondly to the traditions of his younger days, it must be acknowledged that the great bulk of the medical intellect of the present day is agreed that we must regard both these fluids as the most common vehicles of the mysterious agencies of epidemic and infectious diseases. The fallacies which lurked in the supposed discoveries of Hillier, and in the theory of the cholera fungus (which may be taken as an outcome of the reaction above indicated), have no doubt influenced on the side of caution the more trustworthy opinions, as far as they are formed at all upon the subject, yet the experiments of Tyndall and conclusions of Lister have undoubtedly not been without their effect. The bold theory of Pouchet, and the corroborative experiments of Bastian, represent the advanced post of this line of attack upon the hidden secrets of Nature. The establishment of the doctrine of spontaneous generation, or, as Bastian terms it, "abiogenesis," is only wanting to complete the equation of the germ-theory, which, if at length proved, will undoubtedly precipitate the whole of the mysterious haze which now obscures the solution of one of the most difficult and interesting of Nature's problems. Its effects upon the practice of medicine and surgery, especially, would be incalculable, and the satisfaction which it affords to earnest workers towards the light of scientific truth is vividly expressed in every page of the work which heads the list of publications at the foot of the preceding page.

There can be no doubt that if the propositions argued by Dr. Sansom so learnedly, so ably, and so enthusiastically were established, their logical connection with the practical conclusions set forth by Dr. Cumming in the work next on the list would be irresistible. Antiseptic surgery in its most rigid and elaborate development, as represented by some such method as Lister's, must in that case take the place of all others, and must in common humanity be applied in all cases as far as the application is possible. The theory, taken in all its bearings, is complete, all-embracing, and seductive, simple to the understanding, satisfying to the reason, comforting to the patience, and beautiful to behold. Among these attractive qualities are some eminently characteristic of that nude yet concealed Divinity whom we, in this England of this nineteenth century, ignorantly if even fanatically worship. To be simple, satisfying, and beautiful indicate æsthetic qualities which of yore determined the choice of Paris, yet the apple was not given to the Goddess of Wisdom. The object of choice, like Saul's armour upon David, had not then been proved, and we cannot yet say more of the germ-theory, whether with respect to origin, mode of action, or practical consequences.

In reviewing the practical bearings of the theory upon the treatment of wounds, to which we propose to confine our remarks, with the object of comparing them with other methods advocated in the remaining publications on the list, it is necessary to limit ourselves to the results absolutely obtained from the elaborate application of the carbolic and other methods of Professor Lister, as stated in Dr. Cumming's work, with those obtained by Mr. Lund acting upon strictly "antiseptic," as distinguished from "germicide" ideas, and with those achieved by Mr. Callender upon the more common principles of great caution in selection, cleanliness, and a special method of dressing, with a view to complete and continuous drainage, with the application of cotton wool for the object only of warmth and protection.

At first sight the question of the best method of dressing wounds seems to be one open to the most simple tests known among the complicated issues of medical results. We have only to take a certain number of cases of like character, placing them in different wards under strictly the same conditions, to apply to each the system of dressing to be tested for a sufficient length of time, say twelve months, and to note the results. It would seem that if this were done fairly, impartially, and conscientiously, the outcome would declare itself in statistical tables plainly enough.

In glancing over the causes of evil after wounds and surgical



operations, it is at once evident that bad results are far more frequently in the present day owing to erysipelas, pyæmia, septicæmia, and gangrene, than to any other secondary surgical consequences. A review of the opinions lately expressed by some of the leaders of opinion in our profession on the subject of these diseases, at the debates in the Pathological Society, makes clearly evident the greatest diversity of opinion on the most simple facts, not only as to the obscure causes of these scourges, but their comparative frequency in hospitals and private practice, in town and country, and to the manner in which they are originated and spread. It became evident that by very many surgeons, including especially the advocates of the germ-theory, much too great stress was laid upon the local origin of these diseases in the wound itself, and the physicians who took part in the discussions had a fair subject of animadversion upon the limited scope of surgical views upon pyæmia generally.

A similar remark had been previously made in the surgical address at the last meeting of the British Medical Association by Mr. Wood<sup>1</sup> in dealing with this question. That surgeon expressed strongly the opinion he had formed, that "the pyæmic agencies operate in the general system, and if through the atmosphere, in other channels besides the wounded part."

Now, although few surgeons will be found to dispute the position maintained by Mr. Erichsen, that this class of diseases may be generated *de novo* in camps and hospitals from overcrowding, open wounds, and neglect of the well-ascertained principles of surgical hygiene, and may stick for a long time to buildings saturated with the effluvia, yet it is also clear that at times (occurring, apparently, to some extent, periodically), and in some measure dependent, probably, upon atmospheric influences—such cases occur nearly simultaneously in the various metropolitan hospitals, and also in private houses both of the well-to-do and poorer classes. They are, however, as might be expected, and as the medical attendants of the various metropolitan dispensaries well know, more commonly seen in the poorer sort of patients, whose nutrition is sustained more feebly, and whose crowded dwellings are less wholesome than among the upper classes. Now, suppose a number of systems of treatment are compared at a time when such influences are in abeyance, a time not unlikely to occur after a casual outbreak has spent the force of the primary infection and has stimulated inquiry as to the prevention of these diseases, then the results will be probably alike favorable in all. But if they be adopted during or continued through the virulence of an outbreak,

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<sup>1</sup> 'British Medical Journal,' Aug. 9th, 1873, p. 146.

it may well happen that these results may be pretty nearly equally unfavorable, or of equally doubtful value. To compare this with a well-known statistical phenomenon relating to the success of the operation of lithotomy : a surgeon has a long-continued run of successful cases, and reckons his fatal cases three or four per cent. less than other recorded results. This is published, and soon afterwards a quick succession of fatal cases justifies the average law and mortifies the pardonably complacent sense of superiority on the part of the operating factor in the chain of secondary causes.

Such phenomena as these may be, and probably are, dependent upon the sequence of effects originating in the same obscure cause, or it may be a manifestation of that remarkable and mysterious higher and more embracing law which governs other coincidences in hospital events, such as the sudden and nearly simultaneous admission of several similar accidents or uncommon aneurismal and other tumours. These occurrences tend much to the uncertainty of results in purely surgical cases involving open wounds, and when we add to them that class of cases known both to physicians and surgeons, of blood-poison originating in the body of the patient himself where he becomes poisoned by his own secretions, the difficulty and uncertainty attending the testing of the value of any merely external application or mode of dressing become enormously increased. Such cases of self-poisoning seem to be more frequent at times when surgical pyæmia are also prevalent, as was lately seen in the lamented death from this cause of one of the leading London physicians. The remarkable experiments exhibited by Dr. Burdon-Sanderson, at the Pathological Society of London last session, throw some light upon the way in which fluids formed during inflammatory processes, at first apparently innocuous, acquire a deadly and accumulative poison-producing power by being introduced into the living tissues of another body. The facts which that distinguished physiologist has demonstrated so satisfactorily have a suggestive connection, on the one hand, with the phenomena of syphilis and tubercle, and, on the other, with the more intense and rapidly acting animal poisons, against which the antiseptic theory arms itself to do battle. The only satisfactory way to decide upon the claims of a champion remedy is to note with fairness, accuracy, patience, and perseverance, through long periods of time and under all circumstances of hospital and sick room influences, the aggregate results of treatment both with and without antiseptic remedies.

Mr. Callender has boldly entered the lists on the side of the efficiency of the methods of the older school of surgery, somewhat affectedly supplemented by the use of a camel's hair brush

for the scrupulous cleansing of the wound of each patient, and a slightly dramatic parade of careful dressing. This, with absolute rest effected by splints and swings, and isolation and tube drainage for a short time, are, in his opinion, of much more importance than the limited antiseptic treatment of washing with carbolic-lotion and the application of carbolized oil, which he also employs.

His statistical list gives a total of operations at St. Bartholomew's Hospital of 199, out of which six only died. Of these six, two were ovariectomy cases, which, by the almost universal consent of surgeons, rarely do well in general hospitals.

It seems that from this latter fact, first prominently brought into notice by Mr. Spencer Wells's remarkable results, and afterwards adopted, theorised upon, and extended to all operations, obstetrical and others, by the late Sir James Simpson, the first start was made into the train of theory expressed by the injudicious term "hospitalism" of the latter eminent but somewhat too impulsive authority. The conclusions which this theory involved have gone far to produce a feeling which all the acknowledged authority of Sir James Paget, Mr. Prescott Hewitt, and other prominent leaders of surgical opinion was not more than sufficient to remove from the public and professional mind.

But still the position in which Sir James Simpson intrenched himself remains, viz. that what is good for ovariectomy and obstetric operations must be good also for operations generally, and this position clearly is not yet carried.

One of Mr. Callender's fatal cases was the desperate operation of nephrotomy. The three others were less trying cases of lithotomy, laryngitis, and cystic tumour. Of *compound fractures*, upon which Mr. Lister relies much for evidence on the side of antiseptic surgery, Mr. Callender gives twenty-eight cases, in which no death at all occurred. In amputations the success, as recorded in his paper, is equally striking. Of fourteen amputations of the thigh, in which the average success in one fatal case in four or five, he had no deaths whatever. In fourteen cases of amputation of the leg and in two of the arm and in three of the forearm, he had likewise no fatal cases; making in all thirty-three amputations, most of them serious cases, and *no deaths* whatever.

In Mr. Callender's method the use of the drainage-tube is limited to the first twenty-four hours, when it is usually removed. Why the secretions of the wound during that time should be considered more noxious and to be avoided than afterwards it is difficult to understand. Instances are not rare in the experience of surgeons of retention of matter, secondary abscesses and pyæmia



weeks after the first infliction of the wound ; and it may even be considered that the first period of cicatrisation and contraction of the superficial parts of a wound is one peculiarly likely to be attended with retention of matter in its deeper recesses, and by burrowing along the bone, fascia, or muscular sheaths.

We look upon this as a weak part in Mr. Callender's system, and suspect that he has frequently occasion to reintroduce the ingeniously contrived tube, which resembles pretty exactly the winged flexible catheter which is ascribed to Mr. Barnard Holt. We may also mention here that although Mr. Callender repudiates the antiseptic system as altogether unnecessary, he uses both the carbolic lotion, the carbolized-oil dressings, and the carbolized catgut ligatures, upon the strength of which the disciples of Professor Lister would inevitably claim him as a follower under a decided carbolic influence, or with such strong carbolic proclivities which go far to account for his success.

Excepting ten cases under his colleague, Sir James Paget, the cases in Mr. Callender's list were the whole of those occurring under his own care since his appointment as surgeon. The outside world, which has not the data wherewith to calculate, would have been more fully enlightened by the statement of the length of this period, and still more so if the easily conceivable difficulties of medical etiquette and surgical sensitiveness could have been overcome by public spirit, and a comparison made between this remarkable success and that of the other surgeons of the institution not practising Mr. Callender's particular way of dressing. Cases of hernia which, when requiring operation, demand it under conditions which preclude previous preparation and a judicious selection of cases with a preponderating regard to the probable issue, are excluded from Mr. Callender's table. The result of the amputations, of course, may be and probably are to a great extent influenced by such careful precautions. But the compound fractures cannot be so influenced. They must, like strangulated herniæ, be taken as and when they present themselves. They may, therefore, be more safely compared with the results of such cases given in Dr. Cumming's "brochure." Unfortunately these are rather too few in number to be so used, having due regard to the value of statistical results, sometimes impugned even with a much greater sequence of figures. In 12 cases of compound fracture given in Table XII (of which 8 were in the bones of the lower leg), all treated under Lister's method in the Edinburgh Infirmary from September 20th, 1867, to December 23rd, 1869, a period of two years and nearly a quarter, all the patients recovered without amputation. Two of the patients, whose names are unknown, and who were

treated as out-patients, had compound fracture of the radius and of both bones of the forearm respectively; another had compound fracture of the lower jaw. Such cases as these commonly do well under ordinary careful surgical treatment, without special pretensions to influence surgical opinion; and the same may perhaps be said, at any rate in periods of ordinary surgical risk, of the 23 cases of acute and chronic abscess given in Table XII, as well as of the 10 bursal and joint cases collected from the somewhat wide area of Edinburgh, Glasgow, Liverpool, and London, and given in Table XIII. Of these, 4 were ordinary bursal abscesses of the elbow and knee, which very rarely do otherwise than well, and 1 of compound fracture of the thumb metacarpal. One was a case of loose cartilage in knee-joint, which was, we presume, operated on, and only two out of the number were so serious a wound of the knee-joint and suppuration in the same articulation. The remaining six tables of clinical cases in the work, including separately tumours, ligature of large arteries, and in the last four comparatively trifling cases, express in somewhat imposing array the satisfactory results in twenty-eight cases. Of these the five compound dislocations of the ankle-joint in Table XIV are the only operations of which it may fairly be said that they form a good test of a treatment which professes to prevent, by its influence upon the wound itself, the occurrence of pyæmia and profuse suppuration. Ten of the tables, and the illustrations in Dr. Cumming's work, give the experiments upon the resultants of putrescible fluids both with and without antiseptics of various kinds, especially carbolic acid. Such experiments, very extensively and carefully made and illustrated by well-drawn lithographs of Bacteria, Vibriones, Leptothrix, Micrococci, and the rest of the mouldy tribe, which are either the direct causes or, what is widely different, the mere companions of putridity, uncleanness, and the abominable, are to be found in abundance in Dr. Sansom's work. They would, undoubtedly, be much more vividly interesting to the surgical mind, as distinct from the purely scientific and histological interests which are frequently and honorably found associated with it, if they were better proved to be the "*fons et origo mali*" than they actually are, either by the able, elaborate, and ingenious arguments of Dr. Sansom, or the limited number of clinical cases brought forward in Dr. Cumming's work.

From a declared disciple of Mr. Lister, and one more or less, it is to be supposed, influenced by his advice and teaching, we might fairly have expected more actual statistical evidence in order to confound the veteran surgical scepticism which refuses the theoretical position it is advanced to prove, and doubts the



infallibility even of statistical tables unless based on a wide area, and including as large a number of cases as a safe life assurance company deems necessary for its stability.

After all, however, it must be said that the fact mentioned at page 58 of Dr. Cumming's work is sufficiently striking, that no case whatever of pyæmia, no case of hospital gangrene, and only two cases of erysipelas, had occurred in Professor Lister's wards for two years and a half, while, before his appointment to the chair of clinical surgery (in 1866 especially), pyæmia, erysipelas, and even hospital gangrene (so rare in the London hospitals as to be considered almost a bygone tradition) were all very prevalent in Professor Syme's wards. It will be doubted by many, perhaps, whether this beneficial contrast be due to the carbolic treatment as practised by Lister, or to the improved hygienic condition of the hospital generally; efforts in that direction being stimulated by the large percentage of deaths; whether the hospital, in fact, had not at that time got into the chronically bad state of saturation with surgical "fomites," from which the Lincoln Hospital at the present time obtains a bad pre-eminence, in spite of its country air, its salubrious situation, and the carbolising efforts of its medical attendants; or it may have been one of those periodic visitations which fasten sooner upon and continue longer to infest those localities whose conditions favour its existence and propagation in the way which typhoid fever sometimes illustrates.

These doubts could be cleared up in one way only, viz., by a fair unreserved comparison with the results obtained in those wards of the Edinburgh Infirmary (and we believe there are such) in which Lister's system has not been practised during these two and a half years. Individual cases of the healing of large abscesses, after being opened under Lister's method, or of a suppurating knee-joint without the further formation of pus, do, no doubt, affect strongly and favorably the surgical imagination, but such cases have occurred, and it may be added even frequently, without any such elaborated influence, both before and since the Agamemnon of modern Athens; and that the ultimate result may be quite as favorable as can be imagined almost in cases treated on a less elaborate and less ambitious plan than Professor Lister's, is shown in the foregoing results obtained by Callender over a more extensive surgical area than that hitherto published by Professor Lister from the Edinburgh Infirmary.

The tendency to limited views of the cause of pyæmia which we have before remarked in the modern surgical mind is well illustrated by the paper of Mr. Hamilton, the main feature of which is stated to be "the advocacy of the disuse of sutures and



the avoidance of all pressure by plasters and bandages in large lacerated wounds and amputations."

Mr. Hamilton objects to the use of torsion for large arteries because of its uncertainty. He employs the carbolized catgut ligature cut off short, which he (unlike many other surgeons) has found not to cause subsequent trouble. He considers the use of sutures and the close adaptation of the parts of the wound as objections both to Lister's and the Callender's methods, and believes the use of drainage tubes by both to be but imperfect means (in comparison with the disuse of sutures and pressure) for the escape of discharges which may become the seat of morbid changes, and cause danger to the system by absorption.

Mr. Hamilton's pamphlet gives no indications of any particular arrangement of the wound to promote the escape of the discharges, the importance given to which formed one of the excellencies of the methods of amputation proposed by the late Mr. Teale, of Leeds. The value of this is illustrated also by the practice of Callender and Wood. Yet it must, doubtless, have occurred to the author to see that the pressure necessarily caused by the support of the weight of the stump itself frequently puts the force of gravity in antagonism to the escape of the pus, instead of being so adjusted as to act in its favour. Adhesions and scabs frequently form so as to imprison discharges, even when a large wound is left without dressing or even sutures at all, or after the sutures have cut themselves or been taken out.

We see in this pamphlet the prevailing idea that the cause of constitutional effects lies mainly in the secretions of the wound itself. The instances which are daily occurring before the eyes of surgeons of wounds, even large in extent, in a perfectly healthy subject healing without the production of morbid poisons, and often better and more completely, and certainly more speedily, under well-adapted pressure, are left out of sight altogether; while, on the other hand, the occurrence of pyæmia without external wound at all, in what are called medical cases, is also ignored.

In Mr. Lund's pamphlet we find advocated all the careful practice of Lister's method, justified, not upon the theory of germs, but simply upon that of the avoidance of putrefaction and chemical decomposition of the discharges. From the extreme care and personal surveillance which Mr. Lund advocates and practises it is not difficult to account for his satisfactory results. And in this attainment of great cleanliness, as well as complete rest to the wounded part by the avoidance of injudicious meddling and of rough and dirty handling and squeezing, we have the common factor, which can be traced through all the contending systems which claim by their success the approval of the profession at

the present time, and which are fairly represented by some of the works which we have placed before the reader in the present article.

That which comes last upon the list might, perhaps, be considered as falling far short of one of the most important conditions of success. Nothing at first sight seems less clear than earth, and nothing more forcibly to illustrate the aphorism ascribed to the late Lord Palmerston, "of dirt being soil in the wrong place." Nowhere in the world would appear to be a more wrong place for soil and dirt than in proximity to a wound or putrefying sore.

While all the best authorities in this country agree in pronouncing cleanliness to be one thing necessary for surgical appliance, Dr. Addinell Hewson, an American surgical author, advocates a somewhat startling proposition of the direct application of dry earth to the wound, on the ground that the earth is, if dirt, at any rate clean dirt, as the country housewife would call it, that it is absorbent and antiseptic, and contains a great and wholesome proportion of ozone, and quenches putrefaction by absorbing and decomposing its products in the living wound, as it does in the dead body after burial.

The kind of earth which he considers most suitable for employment is the purest clay, for the absorbing quality of which he quotes the respectable authority of Mr. T. Way, the consulting chemist of the Royal Agricultural Society of England, as given in his papers on the "Absorbing Powers of Soil," in the eleventh and twelfth numbers of the Society's journal. The experiments quoted from this author go to prove conclusively the great power of soils of a clayey nature to abstract and permanently retain ammonia, even from its most fixed chemical combinations.

The yellow clayey earth which Dr. Hewson employs he found by experiment to neutralize both alkaline and acid reaction in the wounds and ulcers to which it was applied, to arrest putrefaction and decay, and to absorb all purulent discharges.

The experiments of Boussingault prove that nitrogenized substances, and not those richest in carbon, are those which putrefy soonest, when in the presence of heat and moisture, depending upon the tendency which azote has to unite with hydrogen, in order to form ammonia; putrefaction, therefore, is not simply a process of combustion, which is accelerated, and not stayed by oxidizing agents. Hence an effective disinfectant and deodorizer is not necessarily an antiputrescent, and permanganate of potash has no power whatever over the process which gives rise to the feter which it so promptly destroys. Such agents do not fix the ammonia, and do not prevent the liberation of sulphu-

retted hydrogen, by which the stench is aggravated. Clayey earths containing alumina and its compounds were found by Professor Way to be powerful deoxidizers, while sandy soil was found by Dr. Angus Smith to promote oxidation by allowing of free access of atmospheric air.

The author has made experiments, albeit of a rude sort, which corroborated those of Way—that clay had the power of deodorising putrid urine, manure, and offensive discharges from sores.

Experiments made by Way and Thompson also proved that even ammonia was separated from its solution and held in physical combination by clayey soils. This peculiarity of clay is even made use of in refining sugars by abstracting colouring matter and other impurities which evolve odours, and so rendering the sugar perfectly pure and white.

Upon such theoretical considerations, deduced from the opinions and experiments of various chemists and physiologists, the author explains the action of clayey earth in sweetening and keeping pure external wounds. He gives, in sufficient detail, 103 cases of surgical wounds, compound fractures, burns, sloughing ulcers, fetid abscesses, and the like, treated by dry earth poultices, renewed when the discharges had percolated through them, so as to become offensive by contact with the external air. Many of these cases were chronic ulcers, and other like conditions not of an important nature.

The result most noticed in the account of these cases was that the application of the dry earth was found (notwithstanding the preliminary and natural objection of the patients to the plan of burying them partially in earth before their time) to be finally very grateful to the patient's feelings, allaying pain and diminishing the heat of the part, even below the general temperature. These results were, doubtless, simply from the exclusion of the atmospheric air, and may form a lesson to those surgeons who advocate and practise complete exposure of wounds to the atmospheric influences.

The total result of the cases given by the author cannot, however, be said to form a very encouraging prospect from the adoption of his plan. He had in all thirteen deaths: two from tetanus, one case (of strangulated hernia) died from peritonitis, one case (a burn) died from diarrhœa, and no less than nine died from undoubted pyæmia. These include six cases of compound fracture of the lower extremity, two or three being railway smashes, one a shell wound of the face and knee, and one a wound of the knee by glass. These do not seem more promising total results as regards pyæmia than those of almost any other plan whatever. Upon grounds such as these we can



scarcely recommend or adopt the plan of treatment by dry earth. The author recounts with evident complacency a few cases of direct primary union after excision of the breast for cancer; they are not, however, more numerous or more marked than the instances which occur after the most simple and even the most totally various methods of treatment. He notices several instances where torsion entirely failed in restraining hæmorrhage, and in one the plan seemed to have led to the death of the patient from secondary hæmorrhage. This is an addition to the untoward consequences of relying upon simple torsion of an artery which become public from time to time, and which undoubtedly increase the feeling of additional security which the use of the ligature gives to the mind of the surgeon, and render the universal acceptance of torsion as the most approved method of preventing hæmorrhage less likely to become established.

The impatient disregard of the antiseptic theories which have been from time to time advanced with respect to dressing wounds has led to one of those extremes of practice exhibited by science and art as well as by politics. Not a few surgeons in this country have followed the system which is in full force in the great hospital at Vienna, and have not only discarded dressings altogether, but leave the wound exposed freely to atmospheric influences, and by relying solely on the undoubted advantages of complete and undisturbed rest to the wounded part, have endeavoured to eliminate all those other sources of infection and danger which operate through the hands of dressers and nurses. The results, however, in the great imperial hospital referred to do not seem to be encouraging. Pyæmia and its concomitants abound in that hospital, and by a reaction in the opposite direction have frightened many German surgeons both at Vienna and Berlin into the practice inculcated by Professor Lister. In the Paris hospitals also, where pyæmia had been favoured up to a very recent date by an extreme carelessness in the important matter of cleanliness and ventilation, the institution of Galignani's Hospital on the most approved English plans and under English nursing management has led to a better state of things. The use of carbolic and other antiseptics (which, we must not forget, originated with Lemaire and other Parisian surgeons) seems to be also gaining ground among our ingenious and inventive neighbours. In this, as in many other ideas, they have set the example to Europe, while, at the same time, in accordance with the spirit of scepticism which also distinguishes them, they have been also forward in manifesting disbelief and originating that sort of ridicule which is as much out of place in scientific matters affecting the lives of thousands

of patients, as in matters of religion which influence spiritual life and death. In this, as in some other uncommendable fashions, they have been followed by several practitioners of surgery in this country, whose more staid reputation might lead to the expectation of better things, and whose experience in liberal institutions and in the recently rapid progress of all ideas might be supposed to have counteracted that tendency to take refuge in the haven of finality doctrines, which seems to be the natural goal of opinions when early success has established a complacent self-confidence confirmed by long-continued eminence in a profession of which the chief glory is improvement.

The present condition of the great antiseptic controversy seems to be this. The supporters of the germ-origin of surgical and medical diseases are in a decided minority. This arises mainly from a distrust of purely theoretical considerations as influencing practical treatment, until the theory upon which the latter is founded has been supported by such an array of experiment and fact, and such an accumulation of probabilities, that the induction amounts to a certainty. The germ-theory includes so much more than those diseases of the pyæmic class which are called surgical, and must seek for its proof in the study of physiology, pathology, and medicine, as well as in the more elementary natural sciences which embrace an area so much wider than that with which surgeons are practically conversant, that the latter must perforce wait more or less upon the further development of the inquiries which are now actually in progress in these cognate, though increasingly divergent pursuits. The time for deciding upon the proximate cause or causes of pyæmia as of cancer was shown by the extreme divergence of opinion, and even dissonance of facts, among the most eminent speakers in the late pathological debate on these subjects, *not* to have arrived as yet. "*Quot homines, tot sententiæ*" may be applied here with a force more than usual.

Until that time does arrive it is clear that treatment must necessarily be founded to a great extent upon surmise, and that this will vary with the theoretical proclivities of the practitioner, and to an extent quite as extreme. Among the London authorities at the debate alluded to, the germ-theory was at such a discount that not one could be found to advance it or even to allude to it. Contempt for its pretensions or the fear of the ridicule which weaker brethren might attach to it must have been the cause of reticence so unanimous. A mere uncertainty as to its truth or falsehood could hardly, in an age and a profession so pre-eminently tentative, have effected so curious a manifestation.

But there was no such cold shade over the chemical doctrine



of putrefaction or other similar poisonous decomposition of the discharges or of the blood, which was spoken of and argued upon quite as freely as the necessity for the use of some such theoretical expression evidently required. Among the various ways in which those putrefactive changes and tendencies can be counteracted the use of antiseptics of various kinds (and prominent among them, of course, that of carbolic acid) has quite an even chance with any other.

But cleanliness, rest, and drainage, and ventilation and refrigeration, were far more insisted upon than antiseptics in the latest expression of opinion upon the best local treatment of wounds.

Constitutional treatment in cases where operations are imperative and immediate seems hardly to have been thought of, and the previous careful preparation of the patient for operation in such cases as admit of it, although mentioned and insisted on, is not specified in Mr. Callender's paper. Dr. Sansom's practice of the internal administration of some preparation of carbolic acid seems to have been tried by very few, although the safe saturation of the system with the antiseptic, which can be effected by means of the sulpho-carbolates, seems to be undoubted.

At King's College Hospital this method is being put upon trial in pyæmic cases by Mr. Wood, associated with the respiration of carbolised air, and irrigation with carbolised water to the wounded part. The results of experiments upon an issue so difficult to prove require, however, a long period of careful observation by many competent men before they can be thoroughly relied upon. The course of an attack of acute pyæmia is so rapid and violent in its disorganising effects, that it is probable that no remedies have much power in arresting it when once established. It would seem wisest therefore to rely upon such measures of prevention and prophylaxis as have proved in cases of typhoid and other fevers more fruitful in success than futile efforts at a so-called cure.

Quinine is a remedy which has been recommended, and may easily be combined with other plans. Of itself it has been tried frequently, even in large doses, and has failed to produce any perceptible effect upon the rigors which accompany pyæmia, and it does not seem to exert any decided antiseptic influence upon the tissues.

An important consideration in the estimate of the value of any line of treatment whatever is its universal applicability under all the varying circumstances of human disease or misfortune. The elaboration and care required by Lister's method is no doubt an obstacle to its ready acceptance, and savours of



that peculiar pretentiousness which has been fatal to so many complicated systems of surgical treatment, under the experience that they were seldom necessary even if of some value. To a less extent this feeling applies to Mr. Callender's camel's hair-brush and drainage-tube also. Under the pressure of circumstances frequently exemplified in military hospitals during war, when the conditions against which they are especially directed most prevail, it is evident that they are difficult, if not impossible. Nor can we endow ordinary surgeons and dressers with Lister's or Callender's patient painstaking and prevision. Under many of these circumstances the use of Mr. Hewson's plan of dry earth might furnish its own most powerful justification. Dry clay might be obtainable when carbolic spray producers, prepared muslin, jacquiette, cere-cloth, camel's hair-brushes, and winged drainage tubes could not be procured at all, or not in sufficient quantity. Alternative methods in the practice of surgery obtain often a great value under circumstances admitting of their more ready employment, and one of the greatest endowments of the surgical mind lies in that fertility of resource and recollection which can supply an immediate eclecticism in the choice of the remedies most at hand. But this is and doubtless will remain clear that no antiseptic or other application, nor internal remedies, however valuable in themselves as an aid, will supply the want of surgical skill in arranging by the method of operation or dressing for the due drainage of the wound ; or the want of foresight in the arrangement of the wards and beds ; or the want of cleanliness and care in dressers and nurses ; or the want of judgment in the choice and time of operating, and in the manner of feeding and supporting the "*vis medicatrix naturæ*" which is always and must continue to be the prime factor in restoring tissues and repairing injury, and which has furnished and must still continue to supply the best or sole safeguard of the patient, when man's art, care, and skill are not forthcoming, or fail to do their part towards the sick or wounded sufferer.

The past history of medicine and surgery is full of warnings against learned pedantry and officious prejudice, whether in the name of science or of experience, operating so as to interfere with or to prevent the restorative action and recuperative powers of Nature, so potent and almost self-sufficient in the young and healthy, and so often overweighted and baffled by accident or by misdirected art in the old, diseased, and feeble.

## II.—The Psychology of Communism.<sup>1</sup>

THE intelligent and scientific citizens of Paris are now engaged in what may be called a coroner's inquest as to the circumstances under which the defunct Commune was born, and lived, and under which it died. Much startling and sensational evidence, many conflicting opinions and extravagant theories, have been submitted, and there appears to be a strong tendency to refer the catastrophe either to disease, or delirium, or drunkenness; but the jury has not been charged, nor has a verdict been delivered, nor is there the slightest probability that a unanimous or even satisfactory verdict will ever be delivered. As, however, the court and even the auditory consist almost entirely of physicians and psychologists, and as many of the matters discussed involve medical principles and lead to novel and curious revelations, we have conceived it expedient to give a brief account of the inquiry, even in its present stage. A very partial glance at history shows that wherever the passions and prejudices, the fears and superstitions, the social interests and political relations of men are powerfully stirred and agitated, large numbers of a community in no other way connected with, nay, even at the time in a state of antagonism to the events, may become so excited, extravagant, and infuriated, as almost to fulfil Butler's prophetic question, "What should hinder a whole people from becoming mad?"

When the storm of the Reformation had somewhat subsided there remained a ground-swell and wild currents and whirlpools, which seemed to portend renewed convulsions and devastation. "We read that Munzer and his associate Anabaptists assembled, in 1525, a numerous army, composed chiefly of peasants, declaring war against all laws, governments, magistrates of every description, under the chimerical pretext that Christ was now to take the reins of civil and ecclesiastical government into his own hands, and to rule alone over the nations. . . . A great part of this rabble seemed really delirious, and nothing more extravagant or more incredible can be imagined than the dreams and visions that were constantly arising in their disordered brains" [Mosheim, "XVI Century," chap. iii, sec. iv].

<sup>1</sup> 1. LUNIER, M. L.—*Influence des Evénements de 1870-1871 sur le Mouvement de l'Aliénation Mentale en France*. 'Annales Médico-psychologiques,' Septembre, 1872, Mars, Mai, Juillet, Septembre, Novembre, 1873.

2. LABORDE, J. V.—*Les Hommes et les Actes de L'Insurrection de Paris*. Paris, 1872.

3. COLLINEAU, M. le Dr.—*Les Commotions Politiques dans leurs Rapports avec l'Aliénation*. Paris, 1873.

4. CLÈRE, M. JULES.—*Les Hommes de la Commune: Biographie complète de tous ses Membres*. Deuxième édition. Paris, 1871.

“In the Irish rebellion of 1641, under Sir Phelim O’Neale, the commencement and the causes—religious and patriotic hatred—are clear and comprehensible, but the consummation in universal bloodshed, parricides and infanticides, even among the defeated, conflagration and pillage, are only reconcilable with an outburst of madness in barbarous tribes” [‘Hume’s History of England,’ chapter 55th].

The inhabitants of the Cevennes, superstitious by original constitution, having been deprived of their only educated and trustworthy guides, their clergymen, who had been killed early in the struggle, were surrounded by a large armed force under Maréchal Villars, to compel them to give up the Protestant faith, which had been incorporated with their minds since infancy, or die. They were attacked, their stores pillaged, their homes and hearths made desolate; they were hunted like the wolves on their hills; every social affection and feeling of personal happiness was exasperated; their minds were agitated by the excitement of despair. Under such circumstances it might be predicated, from their tendency to marvellousness, that they would resort to those rites, expectations of Divine interposition, and demonstrations of piety, which the educated and unexcited reject.

Accordingly it is related that they trembled, were convulsed, enjoyed ecstasies, saw visions, and prophesied. At the close of the campaign there are said to have been ten thousand in the field. That all these prophesied is incredible, but that they all believed the prophecies to be oracular is highly probable. Another property which they claimed, and seemed actually to have possessed—that of insensibility to pain—allies their case with that of other classes of men acting under similar impulses. These peasants, during their enthusiasm, spoke in languages unknown to themselves or their auditors. This was not always the case. English, Hebrew, and Greek were occasionally uttered, and likewise occasionally interpreted by the speaker [“*Theâtre Sacrée des Cevennes*,” quoted in Bertrand, ‘*Du Magnétisme Animal*,’ p. 185; and ‘*Smedley’s History of the Reformed Religion in France*,’ vol. iii, p. 52].

In the same fair land of France, some eighty years ago, not only did all government become impossible, not only was society dissolved, and even the family contract broken up, but such gross atrocities, such hideous absurdities were committed, as to have no parallel beyond the arena of semi-civilised or savage races. The hostility of creeds, political and religious, the struggles for gain or glory or power, even the selfish and reckless vindictiveness of party strife, are all intelligible and explicable on grounds consistent with sanity, though not, perhaps, with



sound reason; but the destruction of property, "the feasts of reason," the worship of prostitutes, the dissoluteness of manners, the indifference to human life, both on the part of the murderers and their victims, the toilet for the guillotine among the captives, the decapitation of cock-robins among the street boys, the blood-thirst horribly exemplified in swallowing draughts reeking from the scaffold, and the indiscriminate slaughter of 1,022,351 human beings over a limited portion of France, of whom it appears about one sixth were children; are deeds that must have had their origin in deeper and darker and more unfathomed instincts than are consistent with health and moral responsibility, or have yet been submitted to philosophical analysis. Without approaching the vexed questions of the connate or generic connection of crime and insanity, although these are involved in the discussion, such narratives, as well as many others of similar convulsions, force upon us the conviction that even speculative errors or perversions of opinion may, under fostering circumstances, eventuate in revolting moral depravity or mental obscuration.

These narratives have been selected as instances of social disruption passing into sanguinary outrage or superstitious extravagance, and because they cannot be placed in the category of moral epidemics.

The sensitive French, deeply stricken with shame and horror by the moral bouleversement of 1870 and 1871, naturally recoil from ready and vulgar explanations of such phenomena as might occur to others when dealing with events similar to those epitomised above, and seek a new, or at all events exceptional, solution in a morbid corruption of society, originating in conditions of comparatively short duration.

Although doubting this theory as crucial or exhaustive, we are not inclined to dispute its validity, but conceive that, in estimating the influence of political disorganization, war, defeat, the violation of the sacred soil, we should keep before us that there were many and energetic conservative elements in operation, which should have neutralized all antagonistic tendencies, and which should have successfully opposed the ruin in which the deplorable contest ended. These were—a peace of twenty years' duration, hollow, it might be, but happy in its results; great materialistic prosperity, the cultivation and rapid development of science, especially of the arts of luxury, and, apparently, a subsidence of the passions and ambition which were the characteristics of the first Empire and several succeeding Governments.

On the other hand, if it be considered that Paris, even more than France, had been exposed to causes calculated to shake the

very framework of the community to its base, to eliminate many of the healthy constituents from their legitimate place and influence, and to give prominence and power to all that was morbid, excitable, inflammable, vicious, and dangerous in its uneducated masses; that the sun of the country's glory seemed to set at Sedan, and that from this time a constant migration, amounting after the 4th September to 300,000 persons, including all that was noble, cultivated, refined, stable, and trustworthy, flowed to the provinces or some place of refuge from the capital, where the prolonged presence of the besiegers, from whom all cruelties and barbarities, even massacre, were expected; where there was entire isolation from countrymen, even from the whole external world, and an imperfect knowledge of its doings; where fear, and watching, and hunger, and all the terrible accompaniments of bombardment played a prominent part; and where semi-intoxication, from the modicum of bread soaked in the fatal absinthe, stimulated the avarice, the lust, and all the passions which agitate the outcasts of a metropolis; it would have been visionary to expect a peaceful or a rational issue. Nothing would be easier than to stigmatise all the murderers, incendiaries, petroleuses, drunkards, demagogues, leaders of the Commune and insurgents of Belleville, as a population of madmen, as some of their countrymen have done, or to argue that the ranks of these destroyers were crowded and recruited by individuals actually labouring under homicidal or furious insanity, by pyromaniacs and inebriates, who were the real perpetrators of the outrages attributed to mere political agitators.

M. Lunier, however, has attempted to show, in a series of articles in the '*Annales Médico-psychologiques*,' of which he is one of the editors, that the war and its consequences, and even the violence of the Commune, in place of increasing the number of the insane, although active in the production of mental derangement, were positively accompanied by a notable diminution in the gross number of cases requiring admission into public or private asylums. His statistics, which we shall immediately proceed to analyse, and are not prepared to impugn, are, at first sight, so much at variance with the experience of former observers, as to the influence of such events upon similar occasions, that they suggest the suppositions—I. That many lunatics deserted by or escaping from their relatives during the general confusion and alienation of family ties, joined the contending parties, and were shot or in some way killed during the internecine struggles which constantly occurred. II. That many may have, unnoticed, committed suicide or died from exposure, want, neglect, or disease. We are enabled, from the

evidence of a medical and most reliable witness, M. Brierre de Boismont, to afford a glimpse of the condition of a certain number of the insane protected and supported, in a well-conducted and fairly victualled asylum, and may, from such facts, draw conclusions as to the fortunes and fate of their less favoured fellow-sufferers, who were deprived of all shelter, sustenance, and guardianship. From a letter addressed by M. Brierre de Boismont to the late Dr. Forbes Winslow [*'Lancet,'* March 4th, 1871], we learn that, notwithstanding his foresight and precaution in storing up provisions for several months, including fowls and pigeons for the weaker patients, the inmates, amounting to about 200, were during January reduced to about an ounce of horse or mule flesh per day; inferior black bread, which ultimately consisted of bran, badly sifted wheat, and chopped straw, &c., which could not be eaten even by the hungry; destitute of milk and without fuel, although the cold was frightful, whilst the incessant boom of the bombardment during the night prevented all sleep or repose, and inspired the majority with extreme terror and agitation; and thus an inevitable crisis naturally leading to disease and death was brought about.

At this period the death-rate had risen in Paris from 1300 or 1400, or, according to another authority (M. Lunier), 900 or 1000, the ordinary weekly average, to 4670; and, it is significantly added, "fathers, husbands, and sons, have disappeared mysteriously from the scene of life." III. That inevitably the standard of moral and intellectual insanity would fall very low in the disorder and ruin which prevailed; that the milder forms of aberration would escape observation among the wild and ungoverned populace, while the forms characterised by astuteness, cunning, daring, and desperation, would raise the sufferers into positions of power and trust, or cause them to be regarded as heroes and patriots. IV. That, amid the reigning confusion, the legal steps preceding the process of the seclusion of patients must have been greatly multiplied or increased. M. Lunier, whose statements, however, it must be borne in mind, refer to France generally, although they obtain a melancholy confirmation in Paris during the siege and the Commune, in order to give precision to his exposition of the proportion of the confined insane to the general population, has divided the departments of France into four groups:

I. Those twenty-one departments still (September, 1872) occupied by the Germans, and those only evacuated since July, 1871.



|  |       |
|--|-------|
| From July, 1869, to July, 1870, before the war, admissions to asylums in this region of France amounted to . . . | 2,202 |
| From July, 1870-71, they fell to . . . . .   | 1,533 |
|  | <hr/> |
| Giving a difference of one third, or . . . . .   | 669   |

And of the 809 men going to form this total, after deducting those among them who belonged to the German army, the insanity of 146, or 18 per cent., was due to the events of the war, while the proportion among the females was 12 per cent.

II. The departments, occupied towards the end of 1870 and evacuated in March, 1871:

|  |       |
|--|-------|
| Present in 1869-70 of cases of insanity . . . . .    | 866   |
| And in 1870 to 1871 ditto . . . . .                  | 783   |
|  | <hr/> |
| Giving a difference of about one tenth, or . . . . . | 83    |

Of 440 men included in this second total, after deducting German soldiers, 94, or 21 per cent., became insane in consequence of the events of the war. The proportion among the women was 15 per cent.

III. Of the eleven departments conterminous with the seat of the German invasion, Paris, being included in this number, ten only afforded trustworthy returns, which showed the same number of admissions in 1869 to 1870 and in 1870 to 1871, giving 160 men, out of 819, rendered insane by the events of the war. In Paris, invested from September, 1870, to February, 1871, and subsequently under the domination of the Commune, the admissions were—

|                                  |       |
|----------------------------------|-------|
| In 1869 to 1870 . . . . .        | 2,982 |
| And in 1870 to 1871 . . . . .    | 2,599 |
|                                  | <hr/> |
| Making a difference of . . . . . | 383   |

In order to show the influence of the causes operating in Paris, compared with those in the ten other departments of this group, it is added that, of the total number of admissions, 290 males and 121 females owed their affection to the events of the war.

IV. In the forty-eight departments more or less distant from the seat of the invasion—

|   |       |
|---|-------|
| The admissions in 1869 to 1870 were . . . . . | 4,141 |
| While in 1870 to 1871 they fell to . . . . .  | 3,862 |
|   | <hr/> |
| Giving a difference of . . . . .              | 279   |

Of the 2286 men included in the second total, 209, or 9 per cent., became insane in consequence of the events of the war. The proportion among the women was 5 per cent.

As an indication of the local and irregular operation of the

circumstances under which insanity appeared during the two periods quoted, it is mentioned that in thirteen asylums out of fifty-five no case could be referred to passing events.

The aggregate of admissions into all asylums during the two periods was as follows :

|   |        |
|---|--------|
| From 1869 to 1870 . . . . .                       | 11,655 |
| From 1870 to 1871 . . . . .                       | 10,243 |
|   | <hr/>  |
| Giving a difference of 12 per cent., or . . . . . | 1,412  |

Our author signalises two inferences to be drawn from these figures—(1) that the reverses of 1870 to 1871 produced directly or indirectly 1400 or 1500 cases of insanity; and (2) that, notwithstanding this, there was an actual diminution in the numbers of the insane, or, in other words, of admissions to asylums, nearly to the same amount during the same period. The first of these conclusions is supported by the experience of MM. Esquirol, Pariset, Belhomme, and Brierre de Boismont; the second by observations made subsequent to the Revolution of 1848. Another interesting observation stands prominently forward. During the last six months of 1871, when comparative order was restored, but maintained at the point of the bayonet, the proportion of cases of lunacy began to augment, but only to the extent of 84; whereas, had the ordinary law of progression been in operation, the augmentation would have reached 437. M. Lunier's proposition may be illustrated in another way. In 1869 the gross population of all the asylums in France amounted to 38,519. The gradual annual increase previously observed amounted to 1003. So that, had not the ordinary course of events been interrupted, the total would in 1872 have been 41,528; whereas it was stationary, or rather had slightly decreased, being 38,260. Certain of the indirect causes of this marked change may be approximated by a consideration of the disturbance and parsimony characteristic of the national administration generally; by the consequent detention of patients in public hospitals or at home, where a certain proportion would die and a certain proportion recover; by the economic project of placing in seclusion only dangerous lunatics; by the detention of lunatics in the bosom of their family in consequence of the loss of means—a class of patients actually found to have diminished in a greater ratio than that of the indigent. Entering upon a more speculative view of the subject, we are assured that the neuropaths, or nervous and hypochondriacal inhabitants of Paris, forgot their sufferings or were cured of their delusions during the siege, and that the excitement acted upon those hereditarily disposed to mental affections as a distraction or a counter-agent. In the next place, it is conjectured, that a certain number perished

on the ramparts, or on the barricades under the fire of foes or of fellow-countrymen; and that, as there was more disputation than drinking, especially amongst civilians, the excitable were not precipitated into actual delirium; and, thirdly, that, as the form of disease actually produced by the commotions was acute, the course would be rapid, and speedily terminate in death or recovery. In dealing with the professions and sexes of those affected, it becomes necessary to deduct from the total number of males (588), 195 who were soldiers, and 74 who became insane in dread of being called upon to serve as such, thus leaving 319 individuals deranged in consequence of the events of the war, which, when compared with the number of females (319), demonstrates that the effects of the morbid tendencies was nearly the same in both sexes.

M. Lunier introduces a detailed catalogue of the supposed exciting causes. This long and very minute dissection of the causation of mental maladies originating in the war is valuable, as demonstrating that a large majority of cases were attributable to depressing emotions, especially apprehension; that a reign of terror seems to have pervaded the nation; and, if accepted as involving those who were not pushed on towards alienation, goes far to explain the general collapse of energy and self-possession, as well as of social machinery, which took place. M. Brierre de Boismont, who has lived through three revolutions, made a similar observation in 1848. To have recourse to precise numbers, it would appear that 135 were driven mad by terror of the approach of the enemy, and that six lost their reason during the battles of Sedan and Gravelotte, where various causes must have unavoidably operated. It is worthy of remark in this conjuncture, as bearing a relation to the general panic, that a high tribute is due to the greater courage and higher energy manifested by the women than by the men. Under each of the sections M. Lunier presents typical illustrations, which, if we deduct the predominating number of the cases of lypemania and of those complicated with hallucinations of hearing and sight, possess no special characteristic, and are solely interesting from the exceptional circumstances in which they were produced.

The last entry found in the table given by him is that of three insurgents who were found to be insane; but were we to assume that, in place of this insignificant number, all the lunatics (2599) admitted into the asylums, public and private, in Paris during the siege and the Commune had mingled with and leavened the declaimers of the clubs, the robbers and assassins of the streets, and the national guards of Belleville, no adequate explanation would be afforded of the insane conduct of these different portions of the inhabitants of Paris. That madmen did join



in the revolution and rise to its surface is certain. M. Laborde, who views public events from a very different standpoint from that taken by M. Lunier, draws a portrait of a maniac, a general paralytic, who obtained such unenviable distinction. This wider scope of his work renders it a fitting sequel of that of Lunier, which is exclusively concerned with recognised insanity.

The mind of M. Allix—for we are enabled independently of M. Laborde to supply his name—seems to have been unsettled by the revolution of 1848, and to have passed through various phases of excitement, delusion, degradation, and temporary lucidity. His career is traced through asylums, prisons, conspiracies, prolonged banishment, societies for political reorganization, ambitious and extravagant projects, which culminated in his election to the Commune, and in conduct so erratic and unequivocally insane as to necessitate his removal, first to a prison, and ultimately to an asylum by the fiat and even under the orders of his colleagues. History records many mad monarchs, executive ministers, one Lord Chancellor who was undisturbed by four bottles of port, one poet who drew his inspiration from gin and water, and at least one littérateur to whom opium lent the glorious mirage of his earlier writings, but now for the first time is announced the instrumentality in the attempted reorganization of society of a General Paralytic.

M. Laborde is a disciple of that school of psychology which holds as one of its dogmas that insanity is often associated with the most brilliant genius. He appropriately dedicates his book to M. Moreau de Tours (whose work on *Manie Lucide* may be regarded as the manifesto and charter of this creed), and naturally, and so far legitimately, regrets the misdeeds and misfortunes of the Communists as symptoms of half-madness or psychopathy, of eccentricity, of vicious training, as the work of men who were predisposed to or bordered upon alienation, the more dangerous because they were not entirely deprived of reason, but who, linked together by some common bond or object, presented, as at all epochs they have presented, *collective folly*. This bond or object was, perhaps, a combination of personal aggrandisement and the public weal.

He classes the chief actors in the Parisian tragedies as those—1. With latent but undeveloped mental unhealth. 2. With loss of moral equilibrium. 3. As those instigated by the prodromata of hereditary tendencies, whose parents died in seclusion or whose relatives laboured under unequivocal derangement. 4. Those enfeebled and perverted by intellectual and moral debauchery, by wild fancies and horrors, as well as by crime. 5. With intellectual feebleness and facility. 6. With imitative

disposition. 7. With inordinate and inflamed pride and vanity. 8. Those among the industrial classes impelled by restless ambition and craving for excitement. 9. Those of imperfect and ill-regulated education and blind ignorance. 10. Those acting under the impulses of a wicked and ill-regulated life. 11. Those stimulated by instinctive propensities alone. 12. Those imbued with superstitious theories and expectations, or with religious mysticism. 13. With atheism, different shades of scepticism, and religious errors. 14. Those labouring under dipsomania, partial sottishness, delirium, and intemperance.

The extent to which drunkenness appears to have contributed to the disorder and disorganization under the Commune, and even when the most terrible disasters were impending, cannot be measured or credited by a people like ourselves not celebrated for rigid sobriety, of our neighbours, who are esteemed abstemious, unless by the light of such descriptions as are given by the author, who records sentinels at their posts as so besotted as to be unable to read ; whole battalions of reserve as lying drunk or reeling in unsteady and fantastic dances, even under the destructive fire of the besiegers, and improvised and bedizened officers so intoxicated or muddled as to be incapable of understanding or acting upon the reports brought to them.

Making every allowance for the sensational style of this relation, which has, however, been much diluted and purified, as well as abbreviated, and admitting that it presents only one specimen of what the author asserts as a prevailing state of demoralisation, there are many grounds for concluding that, even during the siege, and certainly during the Commune, the consumption of stimulants was enormous, and that the thoughts and feelings and acts of a large proportion of those actively engaged must have been modified, if not prompted, by partial or complete ebriosity.

The long list of causes, among which intemperance so conspicuously figures, is the condensed summary of 160 pages occupied by M. Laborde in exposing, very pointedly and picturesquely, the source of the mental conditions to which he refers the excesses of the Commune ; but, although it lays bare a most appalling catalogue of the corruptions, perversions, abnormal tendencies, and positive eccentricities which impair the well-being and stability of communities, and which may fairly be regarded as indications of psychical disturbance, we are not prepared to regard them, or many of them, as excluded from that category which comprehends all errors, irregularities, and crimes, or as differing materially from the evil agencies at work in every heart and in every people, and which acquire destructive power and prominence wherever and whenever the restraints of religion, re-



sponsible government, law, and social order, are supplanted by the subversion of all constituted authority.

M. Collineau appears inclined to support this same view—to represent his countrymen, not as insane, but as deceived and misled, and to point to the equal instructions, duties, and rights enjoyed under a commonwealth as the remedy or protection against such disorders. He adopts the orthodox French opinion that the shock of political earthquakes does not increase, but diminishes the number of the insane; that the mass of the population resists and repels the influence of such events; and that it is only such individuals as are prepared by other and adventitious circumstances who tremble or fall under such influences. According to him, we may be under-estimating the magnitude of the evil done, should the slowness of the incubation of many forms of insanity and the fact that attacks occurring now or during future years may owe their origin to the siege, be overlooked. Such caution is particularly necessary, as, even when admitting that national misfortunes lead to explosions of mental disease, the attacks, neither in their character, nor course, nor curability, differ in any respect from those which can be traced to peaceful times and to commonplace causes. He so far corroborates M. Laborde's picture of the prevailing state of intoxication by affirming that after the siege the cases of alcoholism and delirium were quadrupled, and that the subsistence of so many of the inhabitants upon bread moistened in wine necessarily induced sensorial disturbance and intoxication. Subsequent to the month of May the fear of massacre, of the entry of the Versailles troops, and of conflagrations, and the suspicion of imaginary accusations, afforded a groundwork for perturbation and delirium, or that state designated by M. Delasiauve *Pseudo-mania*, in which the mind yields to the dominion of fears and fancies, dangerous in their effect, which are, notwithstanding, known to the intellect to be without foundation. Although advocating a doctrine so nearly similar to that of M. Moreau de Tours, he repudiates the arguments of M. Laborde as to the prevalence of psychopathies or the minor neuroses among the authors and leaders of the Communal movement, and for the following reasons:—1. That the people had remained long unprovoked by an oppressive and defiant Government. 2. That after unparalleled national disasters Paris was converted into an intrenched camp and defended long and bravely under great disadvantages. 3. That not a murmur was heard during the siege under the pressure of the losses from the prevailing epidemics and from starvation. These facts are derived exclusively from the state of the inhabitants of Paris before and during the siege, when comparative order was preserved, and do not apply to the violence and barbarities



under the Commune, and we can only partially sympathise with the indignant protestation against that paradoxical philosophy which would identify the effects of human passion with insanity, or describe the human race as on its confines.

These are mere opinions or inferences, and afford but faint and shadowy representations of the actors in the tragedy before us. M. Clère presents photographs, rigid but faithful outlines, with rarely more colouring than what is required to give individuality and character to the sketch. His pictures include all that he saw or could ascertain of the aspect, bearing, previous history, and tendencies of the individuals he commemorates; his book, written under the very glare of Communal power, suggests rather the potential than the accomplished sway of the temporary sovereigns and sages of Paris, and concludes before the dénouement of their projects or their own fate had been decided. We have examined these biographies very carefully, and are constrained to bear witness to the impartiality and forbearance of the author while describing men from whose objects and deeds he recoiled with abhorrence. Of the eighty-eight members of the Commune which commenced its sway on the 18th March, 1871, the vast majority had undergone, for the discharge of their supposed functions, a preparatory training by affiliation with the International Society, an indoctrination in socialistic and democratic projects in the clubs, conferences, and reunions, which had taken the place of religious, dramatic, even drinking assemblies, and a sort of demoralising process by imprisonment, banishment, exile, and concealment from the police and the agents of the government under which they might be living. Many of these had been committed to prison as conspirators, libellers, as members of unauthorised societies, as offenders against public decency, or against the laws for the time being. These strong epithets are applicable to almost all whose history could be traced; but there are at least seven names which present no antecedents, and several of whom neither the constituents or the Commune knew anything until they commenced public life in that body. Of these rulers of the French capital who attempted to organize Committees for Public Instruction, for Foreign Affairs, &c., and who might have become, had the power to destroy and disorganize equalled the desire, the arbiters of the destinies of Europe, thirty, or more than one third, were from twenty to thirty years of age; Louis Pindy, who set fire to the Hôtel de Ville and Municipal Offices; and Raoul Rigault, who voted for the death of the hostages and then superintended their execution as an amateur cold-blooded assassin, were only twenty-four years of age, and, we regret to add, that the latter was a medical student, though a

Bohemian of the worst cast. Medicine contributed to the ranks of the Commune, for we find one physician, one surgeon, one veterinary surgeon, one quack, originally a priest,—three medical students, and two apothecaries, among their numbers. The remaining members may be roughly arranged into forty-one tradesmen, or as belonging to the uneducated or partially educated classes, and twenty-six as having received either systematic culture or such culture as is imparted in artistic, commercial, or military occupations.

In framing this calculation we have assigned to each individual what appears to have been his original and permanent trade and profession, but each seems in his time to have played many parts, and there are accordingly medical students who have been perfumers; apothecaries who have been newspaper hacks; a Capuchin converted into a commissary of the police; a bath-keeper who figures as an artist, and seven who added journalism to their more legitimate occupations, most generally to advocate their opinions, gratify their revenge, or to promote the cause of their party. Of the eighty-eight, five had been returned to the Constituent Assembly, but declined to retain their seats, and three are described as affluent; but to a large proportion, either in consequence of the loss of pecuniary means, the unproductiveness of labour, or some irregularity in their previous conduct, the allowance of fifteen francs per day must have been acceptable. It must be recollected that almost every member of the Commune was or had been a member of the national guard. What, then, has training—that imparted by the schools, that graven into the mind by the rougher school of the passage through life and through political life—effected in such materials? There were undoubtedly able, though untrustworthy, members of the Commune, such as Deslescluze, Pyat, Varlin, all unfortunately members of the violent faction, and Vallés, who, though moderate, was half-crazed by the ambition of becoming chief of the republic; but of the remainder it would appear that sixteen were vain, pompous, pretentious heroes of scarfs and lace and decorations; that nine were held, even by their contemporaries, to be ignorant, and fourteen incapable; and that twenty-six were moderate, voting against all extreme measures, and especially against the Committee of Public Safety, from which proceeded the arrests, the deaths, the demolition and conflagrations of that sad time. These last constituted the minority of the Commune, and either protested against or retired from the deliberations. But arrayed against these twenty-six moderates were forty-one others advocating violent, cruel, and outrageous steps, either in terrorism, or in emulation of the acts of 1793, or in



realisation of the political views and visions which they entertained. We are not disposed to attach too much importance to physiognomy, but cannot refrain from associating the idea of mental sagacity and serenity with a well-developed and healthy organization, and we have been much struck by the following analysis of the aspect of the members of the Commune. Of these, two appear to have been of unsound mind; one paralytic, the imitator of Couthon of the first revolution, whom he resembled in his infirmities; three were labouring under formidable if not fatal disease; sixteen were ugly, repulsive, or absurd; eleven were diminutive, and two were deformed.

At this point an intense curiosity arises as to the mental condition of the constituencies by which such officials were elected, but all that we can discover is, that Assi, a deserter, and possessing such qualities as to be ostracised by the Commune itself, commanded 18,700 votes; that an eccentric miller, with the aspect and cruelty of a brigand, but who was a popular song-writer, secured 14,000; Mortier, an architect's clerk, with unknown antecedents, who expelled the priests from the churches in order to institute there a propagandism of atheism, carried 19,300; Eudes, uniting in his person apothecary, printer, newsagent, and assassin, who was condemned to death for the murder of a sentinel, and elected to the dignified position of general by the Commune, entered this assembly through the suffrages of 17,300 individuals; and lastly, M. Avrial, a mechanic and soldier in Africa, forced into his position and principles by poverty and misery, was raised into his lofty position as governor of France by 16,100 voters. It would indeed appear, when the different returns supplied are summed up, that more than half a million (560,836) of the inhabitants of Paris chose these men and their confrères as their rulers.

### III.—Quarantine and Epidemic Cholera.<sup>1</sup>

THE publication of Dr. Colin's elaborate memoir affords a

<sup>1</sup> 1. *Quarantaines*. Par M. le Dr. LÉON COLIN, Médecin principal de l'Armée, Professeur d'Epidémiologie à l'école du Val-de-Grâce. Extrait du 'Dictionnaire Encyclopédique des Sciences Médicales,' Mai, 1873. Paris.

2. *Quarantine on the Southern and Gulf Coasts of the United States*. By HARVEY E. BROWN, Assistant-Surgeon, United States Army. New York, 1873.

3. *Abstract of Regulations in force in Foreign Countries respecting Quarantine*. (Parliamentary Paper), May, 1860.

4. *Abstracts of Returns of Information on the Laws of Quarantine*. (Parliamentary Paper), August, 1860.

5. *Papers relating to Quarantine, communicated to the Board of Trade*. (Parliamentary Paper), August, 1861.



favorable opportunity for considering the subject of existing quarantine as a trustworthy means of extrinsic defence against the invasion of pestilence from abroad, and more especially at the present time of epidemic cholera. Although unquestionably one of the most important questions of practical hygiene, and a topic, moreover, that is in itself replete with instructive interest to medical science, it has hitherto been strangely overlooked and neglected by the profession in this country. This neglect is the more surprising, seeing that no nation in the world is so much concerned in the right solution of the many difficult problems to which quarantine is at any moment liable to give rise, either among ourselves or in the numerous scattered possessions of our colonial empire. English literature has, in the present century, contributed next to nothing to the elucidation of the subject and of its manifold bearings on public health and on social and commercial intercourse, except a few official and parliamentary reports, which have never been generally known. Quarantine finds no place as a distinct theme of discussion in any of our medical dictionaries or cyclopædias, and, stranger still, it has not been admitted even into our most recent treatises on practical hygiene, to which it specially appertains.

Very differently has the question been treated in France. Independently of various separate monographs and of numerous notices respecting it in the periodic press, as well as of not unfrequent reference to it at the meetings of the Academy of Medicine, it forms the subject of a distinct article in each of the five professional dictionaries that have been published during the last fifty years in Paris, from that by M. Foderé, in the ‘*Dictionnaire des Sciences Médicales*,’ in 1820, to the present one by Dr. Léon Colin, in the ‘*Dictionnaire Encyclopédique*,’ &c., in 1873. Our brothers across the Channel can also point with just pride to the admirable report of their Academy, “*Sur la Peste et les Quarantaines*,” in 1846, which threw such a flood of light on the whole of the general question, and mainly contributed to dissipate the ignorance and many of the absurd prejudices which had so long prevailed respecting it throughout Europe. It was France, moreover, that moved first, in 1850, to bring together the International Congress that was held in Paris in the following year, and, again, that which met at Constantinople in 1866, to both of which we shall presently have occasion to refer. But before doing this the ground must be cleared of various impediments that stand in the way of the profitable discussion of the general question of quarantine and its relations, so that the reader may have a clear view of the exact nature of the subject presented for his consideration, and may be prepared to judge for himself of the character and value of the evidence upon

which the maintenance of the existing system is generally upheld.

And first as to the exact meaning of the term. In the official language of all nations quarantine is employed to signify the enforced detention and segregation of vessels arriving in a port, together with all persons and things on board, believed to be infected with the poison of certain epidemic diseases, for specified periods of time corresponding with the supposed duration of the incubation or latency in a dangerous state of the morbidic poison. The establishments in which the enforced detention, &c., is carried into effect are called "lazarettoes"—an absurd appellation, only serving to mislead the public mind. But as, more especially of late years, the term "quarantine" has frequently been used in a loose way to denote the simple isolation for a short time on shore of persons who are actually sick, or of vessels which have cases of actual disease on board upon arrival, and as this use of the term will not, it is to be feared, be readily discontinued, we propose throughout this article to designate the original and general thing as "*technical* quarantine," so as to prevent all confounding of it with any of the modifications or mitigated forms of the practice which we may afterwards have to consider.

A retrospective glance at some of the leading occurrences in the history of technical quarantine during the present century will best serve to explain its true character, in a practical as well as in a theoretical point of view. At the beginning of the century the disease which mainly, almost exclusively, occupied attention in connection with the present subject was the Oriental plague. Yellow fever was scarcely yet an apprehended cause of imported danger, and epidemic cholera had not been so much as dreamed of as a frequently recurring scourge, even in Hindostan, far less its occurrence in Europe and in the western hemisphere.

At the time alluded to the strange doctrine possessed the mind of the profession that the general mode of the propagation of the plague was, not by the inhalation of the breath or of the effluvia from a patient actually sick of the fever, as Mead and other physicians had taught at the early part of the century, but by the absorption of the morbidic poison into the system of the person attacked through the skin, consequent upon there having been direct contact, either with a previous patient or with substances presumed to have become tainted with the virus. In short, the ordinary channel of transmission was supposed to be the surface of the body, and not the lungs. Hence recourse was had by physicians and other attendants of the sick to the use of oil-silk dresses and other expedients as the principal



means of protection against the risk of infection in lazaret hospitals, while the free ventilation of the wards and all avoidance of crowding were utterly neglected.

It was on the above hypothetical extravagance that quarantine legislation was then based, to the no small discredit of medical science, the misleading of governments, and the serious damage of public as well as of individual interests. One instance may suffice to show the working of the system. In 1801 two vessels which had arrived in the Thames from Mogadore with cargoes of hides, suspected of being charged with the poison of the plague in Barbary, but without having had a case of sickness on the voyage, were ordered to be sunk in deep water off the Nore, at an expense to the public of £15,000.<sup>1</sup> Such is a specimen of the theory and the practice which continued in vogue when the disastrous outbreak of the pestilence occurred in our colony of Malta in 1813, the retrospect of whose history now, under the light of present knowledge, is so painfully humiliating to the profession.

The crude attempt made by Dr. Maclean in 1817-18, from very imperfect data, to controvert the universal belief in the contagion of the plague, led the House of Commons to refer the question to a select committee.

It examined twenty medical witnesses, the great majority of whom had, however, never seen a case of the disease. The report declared "that the opinions of all these gentlemen, except two, are in favour of the received doctrine that the *plague is communicable by contact only*, and differs in this respect from epidemic fever." The committee acquiesced in this opinion, and added that "they see no reason to question the validity of the principles upon which such (quarantine) regulations appear to have been adopted." A more cautious and much more rational judgment had been expressed the year before by the College of Physicians, whom the Government had consulted respecting the views put forth in Dr. Maclean's work.

"We are of opinion that, although some epidemic diseases are not propagated by contagion, it is by no means proved that the plague is not contagious, or that the regulations of plague police are useless or pernicious. We are persuaded, on the con-

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<sup>1</sup> In a work published so recently as 1846 we read—"I am aware that the expurgation of a plague ship with a cargo of goods is a very difficult, tedious, and expensive operation, as well as one not free from danger. I am nevertheless of opinion that, generally speaking, it will be the best and perhaps the cheapest way in the end to destroy the ship and the cargo at once, and place the people [crew?] under strict quarantine until their state of health is proved to be free from all suspicion."—'A Treatise on the Plague, with Hints on Quarantine.' By A. White, M.D., Dep. Inspector of Military Hospitals, and late Superintendent of the Plague District in Corfu.



trary, that the disease is communicable from one individual to another. . . . The doctrine of contagion appears to us to be wholly unshaken by any argument which Dr. Maclean has adduced ; at the same time, we think it probable that some of the personal restrictions enforced in quarantine establishments might be modified without risk to the public health."

Within four or five years of this date, the then superintendent of quarantine, Sir W. Pym, recommended a great relaxation in all quarantine regulations in this country ; he had never, he said, known, during his long experience, a single instance of a case of plague occurring in a vessel from the Levant, and believed that, with due attention to ventilation, all risk of the spreading of the disease, when it did occur, might be prevented. The progress in ascertaining the real results of actual quarantine experience in other European countries, as well as in our own—obviously the only reasonable mode of determining the value, or otherwise, of the existing practice—was, however, extremely slow, for it took twenty years more before Sir W. Pym could declare that—

"There was no evidence whatever to show that a single case of plague had ever been known to occur in any country from the manipulation of suspected, or declared to be infected, merchandise landed in a lazaret. The records of lazarets also proved that diseases seldom or never spread from even the sick to any other of the inmates, or to the officials of these establishments."

It was not until the publication of the French Academy report, in 1846, that the profession was brought to recognise the simple truth in respect of the main attributes of a disease, the history of which had been so long obscured by the mists of ignorance and prejudice. One of these features has always been that of its frequent or occasional development in various spots in the East, quite independent of any discoverable connection with its antecedent existence in the same or in other localities. There has long been much unwillingness to admit the truth of this proposition, and the most extravagant conjectures have often been entertained to account for the inscrutable appearance of the pestilence in unexpected places, or at unlooked for seasons. Thus, the Malta outbreak, in 1813, was very generally attributed to the purloining by a smuggler (in whose family the first case occurred) of a bale of calico from a vessel which had arrived from Alexandria, but on board of which no sickness had existed (*vide* 'A Treatise on the Plague,' by Sir A. Brooke Faulkner, M.D., &c., 1820) ; while that at Corfu, two years later, was ascribed to the contagion remaining latent in the military hat of an officer who had been one of the victims of the disease in Malta, which had been removed with

other articles after his death to his friends in Corfu (Tully's 'History of the Plague in the Ionian Islands,' 1821).<sup>1</sup>

For the last thirty years, the plague has entirely ceased to appear in the countries where it had long been considered to be endemic, viz. in Egypt and Syria, just as it had done two centuries before in England and other parts of Western Europe, and without any clearly discoverable cause to account for the cessation. But, notwithstanding this notable epidemiological occurrence, there have been, within the last fifteen years, three distinct limited outbreaks of the fever in three separate localities, wholly unconnected either with each other or with any antecedent existence of the disease elsewhere, viz. at Benghazi, on the Barbary coast, in 1857; near to the site of ancient Babylon, and not far from Bagdad in 1867; and on the frontiers of Kurdistan in 1871. Had we no other evidence on the point, these three well-authenticated instances clearly prove the fact of the occasional *spontaneous development*—i. e. development independently of any discoverable or logically presumable connection with antecedent cases of the disease—of the Oriental plague; a circumstance which, while affording no argument whatever against the contagiousness of this disease, is obviously full of significance in respect of its natural history.<sup>2</sup>

And now, as to the past history of quarantine and epidemic cholera in Europe since the first visitation of the pestilence in 1831-32, no one will deny that it has on the whole been hitherto an egregious failure. On the occasion of the second invasion, its practice was given up in England; nor has any one, we believe, ever alleged that this country suffered in consequence, or was then more heavily afflicted than other European nations which repeated their former unsuccessful attempts to exclude or to arrest the scourge. Upon its subsidence, the first international conference of medical and consular delegates, was held in Paris in 1851, and deliberated for more than six months. The result was far from satisfactory, no unanimous report was agreed to;<sup>3</sup> things therefore reverted to nearly their former position. On the third visitation, in 1854-55, the inexorable exigencies of international war brought the working of quarantine

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<sup>1</sup> Dr. White, in the work already cited, "fixed the introduction on a smuggling vessel from the opposite coast of Albania, where the plague was then raging."

<sup>2</sup> 1. 'Une Epidémie de Peste en Mesopotamie en 1867.' Par le Dr. Tholozan, Premier Médecin du Schah de Perse, &c. &c. Paris, 1869.

2. 'Note sur le developpement de la Peste Bubonique dans le Kurdistan en 1871.' Par le Dr. Tholozan, Membre Correspondant de l'Académie de Médecine de Paris. (Extrait de la 'Gazette Médicale'.)

<sup>3</sup> The reader will find an abstract of the leading details of its proceedings in the number of this Journal for July, 1868.



throughout the whole of the Mediterranean, and in the Baltic and North Sea, to a standstill. One result of the enforced suspension of the old system was that it taught us that some places which were, more than almost any others, exposed to the risk of infection from the arrival of infected vessels and the landing of sick persons, were on this occasion, when all "technical quarantine" was discontinued, less severely visited than they had been in former epidemics,—and, we may add, than they have been in subsequent ones. This was conspicuously the case with Malta and with Gibraltar. After the next visitation in 1865-66, a second attempt was made by the European governments to effect, if possible, a solution of the difficult problem of quarantine in respect of cholera by means of an international conference of medical savans and diplomatists. This time it met at Constantinople. Again the attempt proved abortive from the utter want of unanimity among the scientific members on a variety of points, and in consequence of the extravagances of some of the propositions recommended by the extreme quarantinists. As a full analysis of the several reports adopted by the majority has been already given in this Journal, in the number already cited, it is unnecessary to do more at present than to refer to that article. Suffice it to say that the leading conclusions of the Conference were never accepted by the most experienced men in this country, who, on the contrary, very generally coincided with the views expressed in the report on quarantine (based upon a large amount of official information from almost every part of the world) by a committee of the Social Science Association, in 1861, and which will be found in the last of the Parliamentary papers at the head of this article.

The question will naturally occur to the reader's mind, Whence this continual vacillation and discrepancy of medical opinion, so inconsistent with the progressive character of all true scientific knowledge? We have not to go far to find the cause, viz., our faulty method hitherto of investigating the natural history of diseases and their relations to other external phenomena, our neglect of the primary canons of inductive research, and the strong tendency among most medical inquirers to indulge in hasty speculations and theorising? Too often, there is scarce any real attempt made to verify the fundamental data on which speculations respecting the origin and spread of disease are reared. No wonder that theories, resting upon such a sandy foundation, are liable to be ever shifting and insecure. And to make things worse, hypothetical conjectures are not unfrequently made the basis of practical conclusions. Such has been the character of much of the history of "technical quarantine." Let



us glance for a few minutes at some of the fallacies, as they seem to us, on which the whole system rests.

It is, for example, assumed that, because a disease manifests at times the property of contagiousness—*i. e.* communicability, direct or indirect, from the sick to the healthy—this property is an absolute or essentially inherent attribute of its natural history, quite as much so as in botany a poisonous quality is the property of certain tribes of plants, or as in physics elasticity is a property of gaseous substances. But does this view consist with actual experience? Is erysipelas or dysentery, although admitted to be occasionally contagious, at all times and necessarily so? Or can the same thing be alleged of several of the forms of cynanche, which every now and then seem to manifest a decided proneness to spread by direct transmissibility from the patient to his immediate attendants?

The activity of the contagious quality is observed to vary extremely in different years and seasons, according as there is, or is not, at the time a tendency to widespread diffusion of the malady. Diseases very generally exhibit the property more decidedly in epidemic seasons. In non-epidemic seasons, sporadic cases seldom or never show the slightest disposition to spread. Epidemicity, therefore, or, in other words, the “epidemic influence” or “epidemic constitution” of season, in relation to the subject of contagiousness, is not a mere fancy or figment made use of to conceal ignorance, although, it must be confessed, the phrase, like many other terms of medicine, lacks precision and distinctness of definition. But to return to our illustrations. Epidemic catarrh or influenza has often been considered to be truly communicable; but no one, we presume will regard contagion as the main or only element in its diffusion. And so with epidemic cholera. All must admit, we suppose, that in a multitude of instances no traces of any semblance of contagion can be discovered upon the very strictest scrutiny.<sup>1</sup> Some have gone so far as to deny that the disease ever exhibits this property; but surely an extreme opinion in this direction is, considering the strong testimony of several well-authenticated histories of outbreaks of the disease among the residents of a port supervening immediately after the arrival of deeply infected *emigrant* vessels, as much at variance with sound deduction as would be the denial of the

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<sup>1</sup> The modified contagionist doctrine, which has of late years met with so much favour in England, more so than in other countries—*viz.* that cholera is disseminated, not by any morbid exhalations or effluvia from the sick, as in other contagious zymotics, but solely through the medium of the intestinal discharges which, it is held, contain its *materies morbi*—is still a mere hypothesis. Neither experiments on animals nor clinical observations have yet given any satisfactory confirmation of the premiss on which the doctrine is based.

above statement as to the frequent absence of all traces of its contagiousness. If the "logic of facts" is to determine, as it should ever do, our beliefs in such matters, it is only ultraist *doctrinaires* that will withhold, one may suppose, their assent from these simple propositions. Contagion, therefore, seems to be, in respect of certain diseases at least, a conditional and adventitious rather than an essential and inherent property. Nothing can more strongly manifest the extreme views held by technical quarantinists than the universal practice of health officers merely going alongside a suspected vessel in a boat, and professing to judge of the existence, or otherwise, of sickness on board by putting some vague general interrogatories to the captain, without their ever venturing to go up the ship's side.<sup>1</sup>

It is not only in respect of the *spread* of certain zymotic diseases that the above hypothesis—that of contagiousness being an essentially inherent attribute of their nature—has led to many grave errors in hygienic medicine. It is equally so in respect of their *genesis* or origination. From the assumption that the sole or principal factor in the propagation of these diseases is the transmission of its morbid matter from person to person, either directly or indirectly through *fomites*, the idea has naturally arisen that their development must, in all probability, be always owing to the agency of the supposed producing material, supplied, somehow or other, from an actual antecedent case or cases of the malady in question. In the absence of direct evidence, analogy from another department of scientific research has been appealed to in support of this doc-

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<sup>1</sup> As to the risk that would be incurred by a quarantine officer going on board, instead of merely alongside of, ordinary merchant vessels for the purpose of ascertaining the health of those on board upon arrival in port, the following extract from a recent (seventeenth) report of Dr. Dickson, Medical Officer of H.M. Customs, will be read with interest. Speaking of the Gravesend division of Custom House Officers, he remarks:—"As these officers spend their lives on board vessels from abroad, all of which are for a longer or shorter time under their surveillance, their sanitary condition may be considered a fair criterion of that in the port in connection with its foreign trade. Their almost entire exemption from zymotic disease, not only in this, but in preceding years, is very remarkable. Even in the great cholera visitation of 1866, when they were of necessity often living on board ships from infected countries, no instance of the malady occurred among them. Dr. Russell, of Gravesend, who has been in immediate charge of this section of the force for nearly half a century, has hardly ever known an instance of officers of the Customs having been infected from contagion existing in vessels where they have been boarded. And from my own knowledge of the more numerous London division, whose daily business is in ships in dock while discharging cargo, I can fully corroborate his testimony as to the extreme rarity of disease traceable to infection on board ship in a class of men who more than any others are exposed to such risk."



trine, or rather in disproof of its opposite; and it has been imagined that the "spontaneous generation" of a propagable disease is as improbable and as unphilosophical an idea as is the spontaneous production of a vital organism. Here, again, apart from all mere analogical or other conjecture, what does actual experience say on the subject of the development *de novo* of many maladies, which are universally acknowledged to manifest at times contagious properties? Besides dysentery and erysipelas, may we not refer to epidemic ophthalmia, to puerperal and enteric fevers, to diphtheria, to pyæmia, to hospital gangrene, &c., in illustration of the affirmative of this proposition? Even in respect of the plague (the type, as all quarantinists must admit, of a contagious disease), not to allude to earlier evidence, have not the outbreaks of the pestilence within the last eighteen years, already alluded to, shown almost to demonstration that it is liable to spring up of itself at times, quite independently of any discoverable connection with previous cases, either in the places themselves or in other localities?

And does not the past history of epidemic cholera, not only in its "home," but also in this country and on the continent of Europe, afford manifold confirmation of the same position, viz. that local outbreaks of the disease have repeatedly occurred where the most searching scrutiny has failed to trace, or even to reasonably conjecture, any connection with a recent *habitat* of the malady? To limit ourselves only to the experience of our own land in illustration, the first as well as the later visitations of the pestilence furnished very strong evidence upon this point. The sharp explosion in the House of Industry in Coventry, at the beginning of 1838, when not only that town itself but also the rest of the kingdom was exempt, has never been accounted for. And so it was in regard to the isolated outbreaks in 1859—one at Wick, in the extreme north-east of Scotland; another at Glass Houghton, a small village in the West Riding of Yorkshire; and the third at the Coast Guard Station at Southampton. The source of the limited outbreak in the farm-house at Theydon Bois in Essex, in the autumn of 1865, still remains a mystery; neither has the origination of the earliest cases at the East End of London, in the following year, ever been explained, although all the circumstances connected with their appearance were minutely sifted from the moment of their occurrence by very able investigators.

No writer has handled the question of the occasional upspringing of the disease over the area of the European continent, independently of extrinsic importation, with so much ability and in so philosophical a spirit as Dr. Tholozan, of Teheran, whose recent contributions to the literature of cholera deserve attentive

study.<sup>1</sup> It augurs well for the future of epidemiological research that so enlightened a physician continues to carefully watch, in his distant home, the current events relating to the diffusion of the disease over Europe and America, as well as in Asia, which his perfect knowledge of different languages enables him successfully to do.

There is another article in the creed of technical quarantinists that deserves notice, before we proceed to consider the practical side of the question before us, or, in other words, the actual results of the system hitherto pursued. It has always been taken for granted that various dangerous epidemic diseases are liable to be introduced into countries previously exempt, not only by sick persons and others who may have been in contact with them being landed from vessels from infected ports, but also by their cargoes, by the luggage of passengers, and other inanimate substances presumed to have become charged with infective miasms, even when there may have been no sickness on board during the voyage. In former times there used to be generally, and in some countries there is still, a systematic arrangement of articles into such as were deemed "susceptible" or "non-susceptible" of retaining and conveying the morbid poison that was suspected; and specific directions were laid down and enforced for the disinfection of the different substances or "fomites," according to their imagined comparative risk of importing the apprehended disease.

Now, excluding for the moment the special articles of the bedding and clothes of the actually sick, we would ask, is there a single well-authenticated instance on record of an epidemic outbreak of cholera (to which we at present confine our remarks) having been traced to the importation of any description of "fomites?" or is the belief only, like the old traditional stories about witchcraft or astrology, one of the multiform offspring of unreasoning prejudice? Even in respect of the bedding and clothing of cholera patients, the evidence as to the transmission of the disease by them is, it must be admitted, very inconclusive. In Dr. Baly's masterly report, in 1854, we read the result of his scrutiny of the evidence on this and on another kindred points in these words:

"With reference to two other arguments, which, if established, would only prove that cholera is in some cases contagious, the

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<sup>1</sup> 1. 'Origine nouvelle du Choléra Asiatique, ou début et développement en Europe d'une grande Epidémie Cholérique.' Paris, 1871. Masson.

2. 'Durée du Choléra Asiatique en Europe et en Amérique,' &c. Paris, 1872. Masson.

3. *Nouvelles Preuves de l'Origine Européenne du Choléra Epidémique.* Paris, 1873. Masson.



evidence examined has been found contradictory. The frequent communication of the disease by the clothes or bedding of the sick to the persons who handle or wash them, under circumstances rendering other sources of infection than emanations received from the body of the sick improbable, appears to be by no means proved. The preponderance of evidence is, in fact, opposed to its occurrence. On the other hand, the evidence respecting the especial liability of nurses and others attending on the sick to suffer from cholera, although conflicting, is in some instances of such a character as to preclude the absolute rejection of the view that the disease has a contagious property, even although it does not usually spread by virtue of contagion" (p. 222).

In Dr. Farr's official report on the epidemic of 1866 in England it appears, with reference to the relative liability of persons of different occupations to be attacked, that laundresses and washerwomen stood among the very lowest on the list, below ordinary domestics and other servants, far below seamstresses, charwomen, and especially publican's wives. In Paris, too, on the occasion of that visitation, judging from the statistics which have been published, the proportional mortality among the *blanchisseuses* appears to have been anything but high. Out of 300 women employed by the Bureau de l'Assistance Publique in washing and cleansing the linen of the cholera patients, not one had died of the disease; and among 911 persons employed as coffin-bearers or otherwise at funerals, only two had been attacked. These favorable results were attributed to the sanitary and hygienic precautions that were taken. The bed and body linen, and the mattress coverings were always steeped in a solution of chloride of lime, and the same disinfectant was freely used within the coffin and on the sawdust around the corpse.<sup>1</sup> A like result has been observed in India. Dr. Goodeve tells us, in his elaborate article in 'Reynolds's System,' that not one of the washers of the soiled clothing at the large General Hospital in Calcutta had, during a period of twenty-five years, been fatally attacked; and he adds "The same may be said of the dressers and sweepers."

Although the preceding observations may be of some use in considering the general subject of (to coin a cacophonous term) Quarantinology, it must always be remembered that, as quarantine is unmistakably a practical question, its real utility or advantages can only be determined by the results of actual experience in different countries. The case of quarantine is very much like that of the passport system, and the necessity or advisability of either the one or of the other practice is to be settled, not by the discussion or declaration of philosophical prin-

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<sup>1</sup> 'Medical Times and Gazette' for September 29, 1866.

ciples, but by the common-sense simple ascertainment of what have been found to be the effects of the attempts, in time past, to keep out the danger of *ab extra* pestilence on the one hand, or of disloyalty and conspiracy on the other. We shall now therefore examine the subject from this point of view, and for this purpose proceed to briefly notice the evidence thereon that is adduced by Dr. Colin.

“During my residence at Civita Vecchia (1865-6), I could satisfy myself of the immunity of the port at a time when cholera prevailed at Leghorn, Naples, Ancona, and on the coast of Sicily, everywhere imported through communications by sea. This immunity was entirely due to the rigorous and lengthened quarantine imposed upon all passengers from suspected vessels. The prospect of long detention, the minimum being for fifteen days when a case of cholera had occurred on board, had naturally the effect of putting a stop to the arrival of all the Mediterranean steamers. Compare with this the numerous visitations which Algeria has suffered, always imported from France, owing to the insufficient sanitary (quarantine) establishments along the coast. In marked contrast to the experience of our colony was the immunity of the Balearic Islands in 1849,<sup>1</sup> the immunity of Sicily in 1865-66, until the arrival of infected troops from Naples and Palermo to put down the insurrection there;<sup>2</sup> the immunity at the same period of the islands of the archipelago; and, lastly, the immunity, in 1870, of the Turkish ports in the Black Sea in consequence of the quarantine restrictions against all arrivals from the Russian ports.”

With reference to the last-cited example, it is said that, throughout the year named, Turkey remained free from cholera, but that in the following one the disease appeared first in a village on the coast of the Bosphorus (the strict quarantine had been discontinued), and subsequently spread to Constantinople.

Another illustration of the success of quarantine is given in these words:

“In 1866, when all the ports in the Baltic were affected with the disease, 2235 vessels from infected localities anchored in the road-

<sup>1</sup> The Balearic Islands did not escape in the epidemic of 1854, nor yet in that of 1865, when the town of Palma in Majorca suffered severely.

<sup>2</sup> A writer in this Journal for July, 1868, p. 215, remarks—“It is not possible, from the want of reliable evidence, to determine whether the disease was not in the island previous to the landing of troops from Naples, at the end of September, at Palermo. More than one of its seaports had been quarantined by other Mediterranean ports in the course of the summer; and the extreme rapidity with which the disease appeared, according to report, at Catania, and at other places far distant from Palermo, after the landing of the troops accused of having imported the pestilence, is not to be overlooked. Very speedily, nearly the whole of the island seems to have become infected.” No more detailed history of the events has ever been published in any of the Italian or of the French medical journals since the preceding comments were made, so that altogether the commonly accepted statement is at least very doubtful.



stead or in the harbour of Copenhagen. Several cases, some fatal, occurred on board, but the disease did not spread in any instance. During this time, four cases occurred among the inhabitants in the city; one of the patients was believed to have had communication with the sick in the roadstead. They were immediately isolated, and there were no other attacks on shore. Dr. Schleisner, the health officer of Copenhagen, was of opinion that to the quarantine imposed upon the shipping, no passenger from any vessel being allowed to land, must be attributed the all-but-immunity of the city, and the rather so as the pestilence broke out, about the very same time, at Stockholm and other ports in Sweden, which had not resorted to similar protective measures."

Besides these instances, mention is made of the success which attended the adoption of quarantine in the ports of Denmark, Belgium, and of France in 1870-71, when these countries remained intact, although the "epidemic, which had been brooding for some years in the Slavonic countries of Europe, had spread rapidly in the Baltic from St. Petersburg to Hamburg."

Without discussing the details of the several instances now cited to attest the value of quarantine as a prophylactic of cholera, one can scarcely fail to feel some surprise at the extreme meagreness of the evidence that can be adduced in reference to the subject after an experience of more than forty years, during which there have been so many epidemic invasions spread over the face of Europe. Independently of this consideration, it will at once occur to the experienced reader that, on each of these successive occasions, numerous localities and districts, both maritime and inland, in every country that was invaded, remained entirely exempt, although they had the freest intercommunication with infected places all the time. The mere escape of a locality on the mainland, or yet of an island near the coast, on any single occasion or during any one year, cannot therefore be fairly accepted as a sufficient proof of the efficacy of quarantine, and for the simple reason that the exempt locality might have fared as well without such protection. The history of cholera in France, as well as in England, is replete with examples of the sort. Again, the unbiassed inquirer will naturally ask, what have been the results of quarantine in the places and countries where it has been at all times enforced most stringently? and, if it has failed in these, to what causes has the failure been due? Take, for example, the past records of Spain, Portugal, and Italy, where the practice has always been most rigorous in every respect, both as to duration and severity; or take the case of our own colony of Malta, which affords so instructive a comment on the past history of technical quarantine, and of which full particulars

will be found in the numbers of this Journal for January, 1868, page 171, and for July, 1868, page 221. So much, then, for the results of European experience.

The instance cited by Dr. Colin, from New York, in 1865-66, deserves special notice on more than one account, as it afforded, he considers, a striking proof of the efficacy of quarantine regulations against the importation of cholera into a great maritime commercial city,—“e-vitant coup sur coup, en l'espace d'un an, les conséquences de plusieurs importations successives par des batiments infectés au plus haut degré—*l'Atlanta*, par exemple, qui avait perdu en mer 23 passagers, *la Virginia* qui en avait perdu 87, et *l'England* qui en avait perdu 200.” These vessels were, it will thus be seen, not ordinary trading, but emigrant ships crowded with men, women, and children, among whom cholera had broken out with violence during the passage across the Atlantic. This circumstance alone gives them a special character, quite distinct from that of ordinary commercial shipping, and every one will admit that exceptional precautions should be taken under such emergencies. But now the question comes as to the alleged escape of the New York City from imminent risk during a twelvemonth, in consequence of the quarantine adopted at the entrance of its spacious harbour. The facts were these, and the reader can judge for himself:—The *Atlanta*, from London and Havre, having, besides her crew, 64 cabin and 552 steerage passengers, reached the port of New York in the first week of November, 1865. There had been during the voyage sixty attacks, fifteen fatal, of cholera among the steerage passengers. The cabin passengers and crew had been all the time quite unaffected. A strict quarantine was maintained upon arrival for three weeks, during which time several fresh deaths occurred among the steerage passengers, who had been landed on a small island at the quarantine station. The City of New York itself remained then, and afterwards throughout the following winter and spring, until the beginning of summer, 1866, free from any signs of choleraic infection. The history of the events on this occasion was an exact counterpart, in almost every particular, of what occurred in 1848-49, as described in Dr. Baly's report. The parallelism is remarkable. In both instances, the epidemic did not appear in the city until five or six months after the occurrences at the quarantine station in the previous year. In 1866, other infected emigrant ships had arrived before the earliest manifestation of the disease in the city. As all the facts connected with these occurrences have been minutely recorded, we can follow them with exactitude. The *Virginia*, from Liverpool, arrived at the mouth of the harbour on April 18th, having lost



many of her steerage passengers at sea, and with many sick on board. She was treated in exactly the same manner as the *Atlanta* had been. The earliest recognised case of cholera in New York occurred on May 1st; and from that date other cases continued to appear scattered about in bad localities of the city, widely apart from each other. Of the first thirty-one cases, of which twenty-one were fatal, "none were discovered to have been in any way directly exposed to persons or things from quarantine, or from the emigrant landing or depôt."<sup>1</sup> The epidemic continued in New York till November. It is an instructive fact that among the numerous emigrant vessels which arrived during the year, and on board of which no fewer than 604 deaths had occurred during the voyage from Europe, "there was not a single instance of fatal sickness among any of the cabin passengers; also, that not one death occurred among the women employed in washing the soiled linen and dirty clothing of the cholera patients in quarantine."<sup>2</sup>

We come now to notice a very interesting question, which has evidently excited of late much attention among Continental quarantinists, viz. How is it that Great Britain has not suffered much more from epidemic cholera than she has done in the last twenty-five years, during which she has discontinued the adoption of technical quarantine?

"There are countries," Dr. Colin remarks, "in which, notwithstanding their freedom of international intercourse, the cholera seems to be with difficulty importable. Such is England, which appears to enjoy, to a certain extent, the privilege of continuing in epidemic seasons its commercial relations with impunity. There is nothing extraordinary in this, and it would be dangerous to infer from such partial immunities the inutility of quarantine measures against cholera in all places. England knows pretty well that she owes her preservation to her geographical situation."

Now, is it the case that our country has really enjoyed such exemption as is here attributed to her? In the first two European epidemics, and especially in that of 1848-49, she suffered disastrously; and many of the local outbreaks on subsequent occasions, as in that at Newcastle in 1853, have been extremely severe.

That geographical situation is one of the most interesting elements of influence to be taken into account in the study of the natural history of epidemic cholera, in its successive visitations, is quite true; and well would it be for the progress of epidemiological inquiries, if greater attention were always paid to

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<sup>1</sup> 'Report of the Metropolitan Board of Health of New York.' 1866.

<sup>2</sup> 'Report of the Commissioners of Quarantine and of the Health Officer of the Port of New York.' Albany, 1867.

the subject. But we must be cautious in forming any decided conclusions upon the subject, for our authenticated data are as yet neither sufficiently numerous nor exact to warrant our doing so. The history of the disease in the West India colonies of France, as well as in those of Britain, could afford some striking illustrative comments upon this head. While one island was attacked, a neighbouring one, perhaps, escaped, and another more remote was invaded—on some occasions nearly contemporaneously with the first—and this, too, although all were, as well as could be made out, nearly alike, both as regards internal condition and external precautions of defence. But this is only one of the many difficult problems requiring investigation, by the scrutiny of well-ascertained facts in future.

As respects the views on quarantine which have of late years prevailed in England, Dr. Colin is mistaken in supposing that the profession here is opposed to the use of all defensive and prophylactic precautions whatever in regard of vessels from infected ports, even when disease exists on board upon arrival in our harbours. He seems to think that we are, to all intents and purposes, "*partisans de la libre pratique absolue.*" But such has, in truth, never been the case. In 1848 when, upon the recommendation of the late Dr. Southwood Smith (to whom hygienic science is so deeply indebted), the old system was discontinued, it was distinctly explained that, in lieu of the rigorous segregation of infected ships upon arrival, and of the lengthened detention of crews and passengers, there should be substituted the adoption of strict sanitary precautionary measures in respect of the vessels themselves, and of all persons on board, after a due examination by a medical officer. Again, in 1861, the committee of the Social Science Association, in their report to the Board of Trade, after declaring that their object was "to amend and utilise, not to discontinue or abolish, the existing machinery of action," expressly declared their opinion that—

"When quarantine detention is deemed necessary, whether from the actual or recent existence of a malignant disease on board, or from the foul and unwholesome state of the vessel, a careful examination should be made of her, and of all persons on board, by the quarantine medical officer, who should have the power and be charged with the responsibility of adopting such measures as each case demands. The healthy on board need not generally be detained, and the sooner the sick are removed out of the vessel to a suitable locality, where separation from the healthy could be efficiently maintained, the better."

In 1866 the Privy Council, upon the advice of their medical officer, issued instructions to the effect that, after an infected ship has on arrival come to anchor in the place assigned to her,



no person on board should be permitted to leave the ship for three full days, or until the permission of the local sanitary authority has been received.

“The sanitary authority shall” (we copy, with only some slight unimportant abbreviations, from the most recent Government order issued in July, 1873), “as soon as possible after the arrival of any such ship, cause all persons on board to be examined by a legally qualified medical practitioner, and shall permit all persons who shall not be certified by him as sick to land immediately.

“Every person certified to be suffering from cholera shall be removed, if the condition of the patient admit of it, to some hospital or place appointed for the purpose, and no person so removed shall leave such hospital or place until the medical officer shall have certified that such person is free from the said disease.”

When the patient cannot be safely removed,

“The ship shall remain subject, for the purposes of this order, to the control of the medical officer of health, and the infected person shall not leave the ship except with the consent in writing of the medical officer.”

Not to allude to other articles of this Government Order, the neglect or violation of which carries with it a heavy pecuniary penalty, the above extracts suffice to show that all quarantine is really not abolished or entirely abrogated in this country, as seems to be very generally imagined by our Continental brethren. It has only been modified and reformed, in accordance, as we believe, alike with the principles of sound hygienic science and with the teachings of past experience.

This reformed practice might be very appropriately in future designated “*sanitary* or *hygienic* quarantine,” to distinguish it from the “*technical* quarantine” of former times.

Hitherto our remarks have had reference specially, if not exclusively, to quarantine against cholera by sea, or to the risk of the importation of the disease by vessels arriving from foreign infected places; and their general drift has been to suggest the urgent need of a reform of the system as it continues to exist in most foreign countries, and also in our own colonies. But it is not by sea alone that technical quarantine has its traditional rules and practical procedures for safeguard against the apprehended danger. By land, the enemy has been attempted to be kept at bay by equally rigorous and still more summary restrictions. A military cordon has been drawn around the frontiers of an infected district, with the view of preventing, by menace or by force, the flight of all persons who happen at the time to be cooped within the circuit of the dreaded infection; nor has the prohibition been always confined to mere threats. On

various occasions, refugees have paid the penalty of their fear and rashness by being shot, in endeavouring to escape from a focus of sickness and death. Not to go further back than the last ten or fifteen years, instances of this outrage have, there is good reason to believe, occurred both in Italy and Spain. The history of the disease in both these lands abundantly attests the folly as well as the inhumanity of the proceeding.

But, apart from the barbarity of such exceptional acts, the practice of quarantine often gives rise, in the present day and in different countries, to much unnecessary mischief and distress :

“The rigors of the present system” (Dr. Colin remarks) “lead at times to deplorable consequences to many persons to whom it is not justly applicable. With the dread of what may await their arrival in any Mediterranean port, in the event of a death from any disease whatever occurring on board during the voyage, captains of vessels often refuse to take on board all invalids, although their illness has not the slightest connection with any epidemic disease, and requires only change of air or climate to ensure convalescence.

“During my residence at Civita Vecchia I was obliged, in the discharge of my official duties with the French garrison, positively to insist upon captains receiving on board patients exhausted by fevers, hepatitis, or dysentery, and whom it was indispensable to remove out of the locality which was poisoning them.”

All who have had personal experience of quarantine restrictions could adduce instances, not of hardship only, but of positive cruelty, which they or persons known to them have had to undergo, and which every educated physician would condemn as utterly unnecessary on the score of public health.

That on some occasions and in some lands, *e.g.* during pilgrimages in India and elsewhere, a modified system of quarantine on land—consisting in brief temporary detention of the destitute sick and sickening for medical and dietetic treatment, disinfection of clothing, &c.—may be very useful in mitigating, if not in arresting, the progress of cholera, is only but reasonable ; but experience forbids any confident expectations of marked success. In his latest report Dr. Cunningham, the Sanitary Commissioner with the Government of India, declares—

“It cannot be too widely known that quarantine was tried in the hope of protecting a number of the cantonments in Upper India, that in many of them it signally failed, and that in no single instance is there the smallest reason to believe that it was productive of any good. The direct evils of quarantine are great enough in themselves, but many indirect evils also arise from it, and among these, by no means the least, is this—that so long as men believe that they can escape from cholera by such means, they will never be fully



alive to the importance of the greatest safeguard—sanitary improvements.”

The temporary isolation of new prisoners arriving in jails, which has been usefully adopted in late years in the upper provinces under the name of “quarantine,” and the adoption of such precautionary measures in respect of pilgrims, to which allusion has been made above, are, Dr. Colin observes, “very different things from the quarantine spread over weeks and months which was attempted in many places in 1872.” The difference is this—the one was *hygienic* and the other was *technical* quarantine. In a very ably reasoned official report to the Government in 1867 Dr. F. J. Monat, for many years the Inspector-General of Jails in Bengal, had expressed his conviction of the inefficiency of quarantine as a prophylactic there:

“The adoption of quarantine to prevent the entrance of cholera into a town or country is by no means a new measure, and has been tried on so extended a scale without the smallest success that, instead of troubling the Government with any opinions of my own, I will place on record what has actually been effected, the results of the action taken, and the views of some of the greatest authorities regarding it.

#### IV.—Hydrophobia.<sup>1</sup>

THE mysterious and fatal disease known in the human subject under the name of hydrophobia has lately attracted unusual notice in consequence of the occurrence of numerous cases in the vicinity of the metropolis; but it is almost unnecessary to observe that it has always engaged the attention of the medical profession from the singularity of its symptoms and its invariably unfortunate termination.

From almost the earliest ages of recorded history, and certainly in the earliest books on the practice of medicine, the subject of hydrophobia has formed a prominent topic with writers on epidemic diseases; and the horrible sufferings endured by the victims of the malady, and the consequent alarm which its visitations have always caused among the nations of the earth, have called into existence a multitude of supposed specifics for the cure of its symptoms. Fortunately, it may be observed, the disease, though of great and undoubted antiquity, is, or has been until lately, comparatively rare, and hence the

<sup>1</sup> *Rabies and Hydrophobia; their History, Nature, Causes, Symptoms, and Prevention.* By GEORGE FLEMING, F.R.G.S., M.A.I.; President of the Central Veterinary Medical Society; Veterinary Surgeon, Royal Engineers. Pp. 405. London, 1872.

precautions which have from time to time been taken in civilised communities to prevent its communication, while they have been considered, perhaps, by the timid as wholly inefficacious in promoting the proposed end, on the other hand have often provoked the ridicule of those who believe the disease to be wholly imaginary. No one, however, who possesses ordinary reasoning power, or who believes the evidence of his own senses, can come to any other conclusion than that hydrophobia, or the disease which passes under that name, has a real existence, that it annually carries off a certain percentage of the population, and, if contemporary records can be believed, that it is prevailing more extensively at the present than at former periods.

But there is so much mystery naturally associated with the origin and spread of the disease that there can be no wonder experienced at the alarm which its visitations have produced, and the vague and often absurd suggestions which have been offered for its cure. Among other elements of difficulty or doubt may be mentioned the obvious fact that the bites of dogs, which are the most common sources of infection, may be only ordinary injuries, inasmuch as the proof of the existence of rabies in the animal is not always evident; and, moreover, many persons may be bitten, even by rabid dogs, without suffering any evil consequences. But, again, the horrible uncertainty of the result, depending, as it does, on the varying but often protracted period of incubation of the malady, must add another feature of aggravation to the other terrors inspired by dog-bites, while it ought also to stimulate the vigilance of those who are called upon to treat such injuries.

Although, therefore, the leading characters of the disease called hydrophobia are generally well understood by the medical profession, yet the period is opportune for the appearance of a scientific treatise on the whole subject, and we may at once state that, in our opinion, the book now published by Mr. Fleming is in all respects calculated to supply the want which must have been felt by the medical no less than by the veterinary profession, of which Mr. Fleming is a distinguished member. Although, too, it is no part of our ordinary duty to recommend books for the perusal of the general public, yet Mr. Fleming's work ought certainly to form an exception to this rule, for there is so much in his pages which concerns the welfare of the whole community, so much to inspire caution, and at the same time to dissipate unnecessary fear, and altogether such an assemblage of facts and reasonings lucidly arrayed and judiciously commented upon, that we have no hesitation in recommending his treatise to the notice of all who are in any way interested in the subject of the preservation of public health,



either as it affects human beings or those domestic animals which often stand in the light of our companions and friends.

The very name "hydrophobia," we must remark in the first place, is calculated to lead to error, for although, in the human subject, the dread of water is one of the symptoms of the disease, owing to the spasmodic action of the muscles of the throat, yet, in the lower animals, and especially the dog, it is by no means constantly or even generally present, as is amply proved by experience. The opinion generally prevalent that the dread of water is a pathognomonic symptom in the dog may lead, and has led, to the most serious consequences, and Mr. Fleming quotes a case related by Blaine, in which a London physician of eminence pronounced an opinion that a dog which had bitten three persons could not be mad because it was able to drink. Fortunately this opinion was not adopted, and the wounds inflicted by the animal were duly attended to by Blaine, and the wisdom of the precaution was proved by the result, for in five weeks a spaniel which had been bitten became rabid, and a horse which had also been bitten was likewise affected. Other examples are given, but the facts in this particular are now well known, and require little further elucidation.

Mr. Fleming is very emphatic in his statements on this point, and he tells us (p. 199) that—

"It is not true that a rabid dog is hydrophobous. Water does not inspire it with fear or horror, and when it is put before the animal it does not produce aversion. From the commencement to the termination of the disease there is no antipathy to water. The many hundred rabid dogs seen by Blaine, Youatt, and others, did not evince any marked aversion to that fluid; on the contrary, the animal is generally thirsty, and if water be offered it will lap it up with avidity, and will always swallow it at the commencement of the disease. When, at a later period, the constriction about the throat—symptomatic of the malady—renders swallowing difficult, it does not the less endeavour to drink, and the lappings are as frequent and prolonged as deglutition is retarded. Even then we see the suffering creature in despair plunge its entire muzzle into the vessel and gulp at the water, as if determined to overcome the convulsive closure of its throat by forcing down the fluid . . . So little dread have the canine species of water that they will ford streams and swim rivers, and when in the ferocious stage of the malady they will even do this in order to attack other creatures on the opposite bank."

A painful case is quoted, on the authority of a French writer, in which a lady who owned a greyhound, which was in the habit of sleeping under the bed, observed one morning that the animal, in addition to tearing and gnawing the coverlet, *drank a larger quantity of water than usual*, though it ate little. The

lady, alarmed at this change in its conduct, consulted a veterinary surgeon, who, however, did not find anything to cause anxiety; but on the next day the animal wounded her slightly at the end of the finger when she offered it some food, and the day after this event it died, and “*had never ceased,*” says the narrator of the case, “*to drink very copiously of water until the end.*” The lady was bitten on the 26th of December, and on February 4th, in the succeeding year, she was seized with hydrophobia and died on the 7th.

The foregoing remarks are not only most important in themselves, but they serve to explain the title given by Mr. Stewart to his book, ‘*Rabies and Hydrophobia* ;’ the latter word being retained, although still somewhat improperly, to denote the disease in the human subject, and the former, which is the word used by the Latin writers, being applied to the malady in the lower animals. Pliny, Cicero, Horace, and Ovid, used the word *rabies* in this sense, but the ancient Greeks termed it *lyssa* or *lytta* (λυσσα), although Celsus, in describing the disease in his fifth book, says that the Greeks call it *hydrophobia* (ὕδροφοβίαν, Græci appellant), and he states that it is characterised by the patients being afflicted at the same time with thirst and fear of water (simul æger et siti et aquæ metu cruciatur).

Mr. Fleming treats of his subject in successive chapters, beginning with the history of the disease and its geographical extent, and then he proceeds to discuss its etiology and its symptoms, the morbid appearances found after death, the nature of rabies and hydrophobia, the medical and preservative treatment, and the preventive measures to be adopted in cases of bites from rabid animals.

The history of rabies and hydrophobia may be traced to a pretty remote antiquity, and, unlike many other diseases, the descriptions given by the ancient writers are so distinct as to leave no doubt as to the nature of the malady they are describing, however much they may overlay their descriptions with absurd hypotheses as to its nature, or with still more absurd recommendations as to its prevention or cure. It is only justice to Mr. Fleming to remark that in this historical summary, which must have entailed great learning, labour, and research, he has invested a very dry subject with a great amount of interest, and it is also only fair to mention that in the quotations introduced, which comprehend passages from or references to Greek, Latin, German, and French authors, there are hardly any errors, and these few are apparently only typographical ones.

Homer is supposed, with some probability, to have referred to the disease under consideration from a passage where an epithet is applied by one of his heroes to Hector, who is there compared



to a raging dog (*κυνά λυσσητήρα*), and from an examination of other passages. Aristotle appears to have been acquainted with it, although he describes the disease as fatal to all animals which are bitten, *except man*; and Hippocrates is believed to refer to it in one or two passages, which, however, are not very clearly worded. Celsus, as has just been mentioned, has certainly described it, and seems to have made a special study of it, and was fully aware of its fatal character, for he states that there is but little hope for the patients who are attacked. In the treatment of the disease, also, this author offers some most excellent and valuable suggestions, such as caustics, burning, cupping, and also the sucking of the wounds of those bitten by rabid dogs, and he also endeavours to show that there is no danger attending this last operation, although it appears to be recognised to be essential to safety that there should be no sores or abrasions on the lips or mouth of the operator.

Cælius Aurelianus, Pedanius Dioscorides, Paulus Ægineta, and other ancient writers who are more or less known, have described the disease with considerable accuracy, and there is conclusive evidence that the malady was formerly frequent and serious in England, for in some Anglo-Saxon manuscripts the writers advise remedies or incantations to cure or exorcise it.

It would be a waste of time to describe any of these supposed remedial measures, although their very absurdity is amusing, and we mention, of course only to condemn it, the ridiculous practice of *worming* (as it is called) the dog's tongue—a measure which seems to have been long held in esteem, and which is still, in some quarters, regarded with favour. It is, perhaps, almost unnecessary to state that the supposed *worm* under the tongue is merely a fold of mucous membrane, and that its removal can have no influence whatever in preventing or curing hydrophobia or any other disease. Equally absurd, of course, was the idea that the disease was to be cured by the miraculous operation of the priests in mediæval times, although we find that so late as the year 1671 there was a rubric of regulations published for the guidance of those persons who were taken to be cured at the shrine of St. Hubert, and no less a philosopher than Van Helmont appears to have been a believer in the virtues ascribed to this saint's interposition and in the efficacy of the practices imposed by the priests.

In the seventeenth and eighteenth centuries it is abundantly proved by authentic documents that rabies prevailed in various parts of Europe, including England, and in other parts of the world, as, for instance, in North and South America and in the West Indies. In the nineteenth century the disease seems to have become much more frequent than in the previous eras,

especially in France, Germany, and England, and also to have extended very considerably its geographical limits. In 1819 the Duke of Richmond, when Governor-General of Canada, was bitten by a fox, and died of hydrophobia; and numerous records exist in proof of its ravages in Holland, Sweden, Russia, Saxony, and many other countries.

With regard to the geographical extent of rabies, Mr. Fleming remarks that, although the disease is undoubtedly most prevalent in the temperate regions of the world, and very much less frequent in the torrid and frigid zones, and altogether unknown in some parts, yet the increased facilities lately given to locomotion between different and hitherto remote quarters of the globe have tended to diffuse and generalise the malady, and to introduce it into countries where formerly its presence was not felt. Still, it is a curious fact that, notwithstanding the most diligent research, it is not ascertained that the disease has even been witnessed in Australia or New Zealand, although, of course, the number of dogs imported into those countries must be very great, and a similar immunity is said to exist in the Azores and St. Helena. In Africa it seems until lately to have been rare or altogether unknown, and Dr. Livingstone believed that it was absent, as a general rule, not only in Africa, but within the tropics generally, which opinion is proved by subsequent experience to be erroneous. The colder regions of the globe are not exempt from the ravages of rabies, though in the very coldest it is but seldom observed. Thus, in Sweden, Denmark, Norway, Russia, and Lapland, it has been frequently seen in an epizootic form, though in Greenland and Kamschatka it is said to be quite unknown.

But in temperate countries there can be no doubt that the disease is always present, and in an unmistakable form, and Europe appears to be particularly affected with its ravages, which, Mr. Fleming thinks, are increasing in virulence and frequency. Among the nations of Europe, however, some regions appear to be more afflicted than others, France and Germany and Upper Italy and Holland seeming to suffer more than other Continental nations; and in Great Britain, England seems to suffer much more severely than Scotland.

The etiology of rabies is a very important question, and at the same time it is one very difficult of solution. Hydrophobia in man is undoubtedly due to the bites of rabid animals, and so far the question is easily answered, but there is very great difficulty in tracing the origin of the disease in the lower animals, although even in them the usual cause is a bite from some other creature. The spontaneous origin of rabies in the brute creation has been strongly denied by many authorities,



who contend that the disease is propagated and maintained only by the transmission of a specific virus from the diseased to the healthy, and this view is, no doubt, that which is usually entertained. But Mr. Fleming observes that, although in the case of the human subject the general opinion may be correct, it is impossible to apply the same reasoning in comparative pathology. He tells us that numberless facts, in almost every day's veterinary experience, appear to demonstrate that various diseases in the lower animals may be developed directly without the intervention of any infecting medium, and he feels justified in asserting that rabies must be included in this category as a disease which, generated under certain favorable conditions, develops a virus which, like a ferment, produces characteristic morbid changes when it gains admission into a previously healthy body. It is true that many distinguished veterinarians, such as Blaine and Youatt, have affirmed that rabies is entirely due to a traumatic action, namely, the bite of some rabid creature inflicted on another previously free from the disease, yet the present opinion is that it will occasionally appear in a spontaneous manner and without any assignable cause. Still, the transmission of the disease by inoculation furnishes by far the largest number of cases, and the positive assertion of its spontaneous development must be qualified by the admission that in the brute creation the fact of transmission can often be neither confirmed nor denied, besides which the long period sometimes, and indeed often, occupied in the incubation, must add another feature of difficulty and doubt to the consideration of the question at issue.

Mr. Fleming adduces a number of interesting facts and arguments to show that rabies may be generated spontaneously, and mentions, for instance, the occurrence of the disease in countries and at periods where and when it had never previously been known; and it is urged that, knowing the extreme limit of the incubating period in the canine race, it is unreasonable to pretend that the malady can lurk unobserved in a dog for ten years, or even for one year, and yet this must be admitted if, in the face of recorded facts, its spontaneous origin be denied. Again, the malady is frequently observed attacking dogs which have been isolated for months together on farms, chained up in courtyards, or kept in rooms as lapdogs, and having no communication with other animals of the same species. It is also argued that if the disease owed its diffusion only to rabid animals, it ought to be always present in certain countries, whereas in these very countries where it chiefly occurs it appears in a general or epizootic form only at certain intervals. From a review of all the circumstances, therefore,

Mr. Fleming is constrained to agree with those veterinarians who maintain that rabies may be spontaneously developed, and that though the spread of the malady among a large number of animals, over a wide extent of country, may be largely or even entirely due to inoculation, yet that in these circumstances there is also some mysterious influence at work which might be designated "the epizootic constitution of the disease." But he justly adds, that the spontaneous origin of the malady cannot be directly proved, because the proofs which are essential to establish this position with the necessary rigour and precision are wanting; belief on the point must be founded on collateral circumstances, which, nevertheless, favour the view here advanced.

As to the cause of rabies, Mr. Fleming confesses that it has yet to be elucidated, and that it may be said we are in complete ignorance of the conditions on which its spontaneous production depends; nevertheless, he enters into a careful investigation of all the circumstances which from time to time have been regarded as accounting for its appearance, more particularly in the epizootic form.

This part of the subject we may pass over lightly, because the result of inquiries and statistics is for the most part negative. Climate, as has been shown, has but little influence, although it seems that, on the whole, temperate climates suffer most from rabies, and those regions which are exposed to extreme heat or cold generally escape its ravages. In reference to the influence of season, it is only necessary to adduce facts in opposition to the common notion that rabies is particularly prevalent at certain periods of the year, which have been therefore absurdly designated the "dog-days," in which it is even now generally believed that dogs are particularly liable to be attacked with madness, and in which precautionary measures have been especially, if not exclusively, recommended and enforced. But in contradiction to this popular view, which seems to have descended to the present day from some fanciful notions prevalent among ancient authors, it is shown from statistics that, in France and Germany at least, it is not during hot weather that rabies is most prevalent, but in cold and mild seasons, and although in other reports it is shown that in certain months the disease is more prevalent than in others, yet the differences are so entirely independent of any fixed law that the conclusion may be fairly drawn that meteorological conditions and the different seasons have very little effect in exciting or determining the evolution of spontaneous rabies, and that the disease may appear in any season or in any kind of weather. In foreign countries much more attention is paid to



the existence or progress of epizootic maladies than in our own, and we are informed that Professor Röhl found canine rabies prevailing in an epizootic form in the years 1814, 1815, 1830, 1838 to 1842, and in 1862, in Vienna, although the meteorological characters of those years were very different; and Professor St. Cyr, who devoted many years to the study of the disease, does not hesitate to say that cold, heat, drought, and humidity, and indeed all meteorological influences whatever, are absolutely without any perceptible action on the development of rabies, and as a remarkable fact in corroboration of this statement he shows that the two months of April and September in 1865 were remarkably dry and warm at Lyons, although in that city they are usually wet; but although the cases of rabies might be expected to be equal, if hygrometrical conditions influenced the disease, yet in April there were fifteen mad dogs and in September only one.

With regard to other circumstances in connection with the etiology of rabies, the results are for the most part also negative. Thus, hunger and thirst, putrefied food and bad water, although sometimes assigned as causes of madness in dogs, appear to be incapable of causing the disease, for these animals have been experimentally subjected to these influences without any other result than death from inanition and exhaustion. The influence of sex and the generative functions are likewise insufficient to explain the production of rabies, although it would appear that dogs are more liable to the disease than bitches, and there seems to be some slight evidence that the enforced continence of the animals is a predisposing cause of the affection. But the evidence adduced on this latter point, derived from a comparison of the number of cases occurring in civilised communities where the animals are in a state of restraint, and of those where they are allowed to wander about freely, is met by facts showing that dogs leading a nomadic life are quite as liable to rabies as their domesticated fellow-species. Indeed, some authors maintain that the domestic dogs which become rabid are really affected by the bites of wild or stray ones, and the fact that rabies is developed in wolves and foxes which live in a savage state, where their desires are uncontrolled, forms a conclusive argument against the supposed influence of continence as a cause of the malady. Again, it has been stated that anger may be a cause of rabies, but it is justly urged that, if such were the case, rabies and hydrophobia would be met with every day, and, moreover, the promulgation of such an opinion might spread an infinite amount of unnecessary alarm in all civilised communities, where bites from angry or ferocious dogs are of very frequent occurrence. The influence of pain is equally insufficient to

account for the disease, and, in reference to breed, there does not seem to be much difference in the kind of animals attacked ; as to age, it would appear from statistical evidence that the dog is liable to rabies from the earliest to the latest period of its life, but that, on the whole, the age from one to seven years is that which is most susceptible to the invasion or generation of the malady.

Thus, then, although the spontaneous origin of rabies in the lower animals cannot absolutely be denied, it is almost impossible to assign its origin to any specific cause ; but Mr. Fleming thinks the disease is certainly on the increase in the great centres of civilisation in Europe, and its development would seem to coincide with the degree of domestication and over-refinement or “ softness ” of the dog, the life which it thus leads being directly opposed to the conditions proposed by nature for the existence of carnivorous animals.

All authorities are agreed that the disease is contagious when once it is present in an animal, and that it is propagated by the presence of some deleterious principle passing from one body to another by various modes of communication. Of the nature and composition, however, of this animal poison we hitherto know nothing, except that it is organic matter in a peculiar condition, capable of transmitting disease from one body to another. The vehicles of its transmission are various, though the saliva or mucus from the mouth of a rabid animal is universally and justly considered the most frequent medium. Experiments have been made as to the influence of other infecting agents, some with positive, others with negative results ; thus, the blood, the flesh, and the milk, have been in certain cases shown to become the means of transmitting the poison. Eckel, of Vienna, and Lafosse, in France, have produced the disease by inoculation of the blood taken from rabid animals ; but, on the other hand, Breschet, Magendie, and Dupuytren, have transferred the blood of rabid animals into healthy ones without producing the malady. The influence of the *flesh* of such animals, when taken as food by other creatures, is at present a subject of doubt, some facts seeming to show that the consumption of such food is innocuous, and others pointing to a different conclusion. The influence of *milk* is also still involved in doubt, but the preponderance of evidence would seem to prove that the milk of rabid animals is not injurious when used in the ordinary modes as an article of food ; but it is still a question whether this liquid would be equally innocuous if drawn immediately from the mammary gland and brought into contact with any other absorbing surface than that of the digestive organs.

Dogs and cats, as is well known, transmit rabies to other



species of animals and to one another, but it has not been clearly understood in recent times whether the poison in other kinds of the brute creation was equally transmissible. Sir Astley Cooper and Mr. Coleman, the veterinary surgeon, thought that the power of propagating the disease was confined to those animals which naturally employ their teeth as weapons of offence, but late experiments have proved this opinion to be erroneous, for a dog has been successfully inoculated with the saliva of rabid herbivora, sheep from that of a mad ox, and a horse from that of a rabid heifer. From all the facts which have been collected Mr. Fleming thinks it may be safely affirmed that the virus of every rabid animal will communicate the disease, and that probably all warm-blooded creatures are susceptible of its influence.

Another very important subject of investigation is the frequency with which rabies follows the bite or inoculation with the infecting material of a mad animal. All animals bitten do not contract the disease, as has been conclusively shown by Renault, of the veterinary school of Alfort, for that veterinarian caused some dogs, horses, and sheep, to be bitten several times by rabid dogs in his presence and on those parts of the body where the skin is thinnest and has least hair, and he also inoculated healthy animals with the saliva obtained from the mouths of mad dogs when they were most furious. The number thus experimented on was ninety-nine, of which sixty-seven contracted the malady, but the other thirty-two were not affected, although they were kept under observation for more than a hundred days. In reference to the human subject, the same comparative immunity has been proved in many cases, and it is consolatory to reflect that many persons who have been injured by rabid dogs, cats, wolves, foxes, or other animals, have nevertheless, remained unaffected; but it appears that the injuries caused by wolves have been most frequently fatal.

Several instances are given by Mr. Fleming from British and foreign authorities, showing the percentage of those who have not suffered any bad consequences although they have been bitten by animals manifestly rabid, and it is estimated that the number of persons who are affected with hydrophobia, after being bitten by rabid dogs, is about 30 to 40 per cent. These results have, of course, a very important bearing upon the question of the supervention of hydrophobia in man after the bites of rabid animals, because it is thus shown that, independently of the doubt whether the animal was rabid or not when the injury was inflicted, it is moreover proved that even the reception of the poison does not necessarily involve serious consequences.

The male sex in man appears to be more liable to hydrophobia

than the female, owing, probably, to the fact that the dress of the latter protects the wearer better from the risk ; the age most liable to the disease, as shown by statistics, is that of very early life, as from five to fifteen ; but, on the other hand, though more are bitten at this period, more proportionally escape ; and the parts of the body most likely to receive the virus are those which are uncovered, as the hands, the face, and the lower limbs.

As to the rate of mortality from hydrophobia in different countries and in different years, Mr. Fleming gives a number of interesting statistical tables, from which, however, no definite numerical conclusions can be drawn. The total of cases in proportion to the population, however, is happily shown to be comparatively small, at least up to the date when Mr. Fleming wrote and completed his book. He estimates the mean annual rate of mortality from hydrophobia, in England, for fifteen years, as  $\cdot 5$  to every 1,000,000 inhabitants. But the figures on which this calculation is based (from the returns of the Registrar-General) show a very great disproportion in the number of cases for the different years, and up to the year 1869 it would almost seem as if the malady was on the decline, for in 1866 there were thirty-six cases, in 1867 there were ten, in 1868 seven, and in 1869 there were eight. But no correct inference can be drawn from these numbers, for it is well known that since 1870 the disease has appeared more frequently than before, and in the years previous to 1866 the number of annual deaths from hydrophobia, in England, exhibits the greatest diversity, ranging from one and two up to twenty-five and more. The maximum, in England, was thirty-six in 1866.

A serious and important question, but one very difficult of solution, is the proportion existing between rabies and hydrophobia, that is to say, between the number of rabid dogs in a given community and the cases of the disease in the human subject. In England no statistics of the number of rabid dogs at any period have ever been collected or preserved, but in Paris the number have been pretty accurately registered, and from the data thus afforded it is found that there was in four years an annual mean of nearly forty-eight mad dogs in two great canine establishments, while in the entire department in which Paris is situated the average number of deaths from hydrophobia was only  $2\cdot 35$  a year. It is clear, therefore, that the victims of the disease in man are far smaller in number than they are in dogs, and the result is supposed to be largely due to the fact that the mad dog manifests a desire rather to wound its own species and to avoid injuring man.

One of the most difficult and painful problems in the history of hydrophobia is to determine the period of incubation of the



disease, for in no other malady is the interval between the reception of the poison and the manifestation of its effects more variable and uncertain. The wounds inflicted by rabid animals speedily heal up, but the disease may break out long afterwards, and, in connection with this part of the subject, it must not be forgotten that the influence of the imagination and the terror inspired by the probability of contagion sometimes produce the most distressing nervous disturbance where no disease really exists, and in cases where none will in all likelihood supervene. The question is interesting in another point of view, namely, as to whether the latent disease, which might under other circumstances never have become developed, may be induced or brought into activity by terror or agitation of mind. Some striking cases, which seem to show the probability of such an event, are quoted by Mr. Fleming, who also relates a curious fact which came under his own notice in reference to the disease in the dog. A very small toy terrier of his own, with which he was travelling in a railway carriage, was suddenly terrified by a railway whistle, and the little creature became mad and died in a few days. It is doubtful, as Mr. Fleming observes, whether this dog had been previously bitten by some rabid animal, or whether it was a case of spontaneous rabies caused by the accidental circumstance alluded to. The incubatory period in man appears, from the investigation of a number of cases, to be from thirty to fifty-nine days; and although John Hunter said that the longest period was seventeen months, and some foreign writers have extended the period to ten or fourteen years (!), yet Mr. Fleming is probably right when he agrees with Trousseau in believing that the disease generally shows itself in man in from one to three months, and in doubting the authenticity of cases where the disease is said to have occurred a year after the person had been bitten. Bouley, who is a very competent authority, considers that the danger of contagion diminishes considerably when two months have passed since the infliction of the injury, and that beyond the ninetieth day the chances are in favour of complete immunity.

The symptoms of rabies in the dog as well as in other of the lower animals, as the horse, the cow, the sheep, the goat, the pig, and in poultry, are most carefully and elaborately described by Mr. Fleming, and if we pass rather lightly over this part of the subject it is only from want of space. As to the dog, which is the most frequent source of hydrophobia, it is clearly a matter of vital importance to determine whether in any given case it is the subject of rabies or not, as the lives of many human beings may depend upon the diagnosis, and it is almost

needless to observe that popular views on this subject are often very erroneous, and the practices founded on those views not only erroneous but dangerous. It is shown, for instance, that the common notion of a dog being afraid of water when it is rabid is altogether a mistake, and it should also be mentioned that even violence is not necessarily characteristic of the disease, for there is a variety of it called "dumb madness," in which the poor brute is rather an object of pity than fear, but in which the danger is as great to man or other animals as in the "furious" variety. Among the most important symptoms so ably grouped together by Mr. Fleming it would appear that the sound of the bark is very characteristic, and, indeed, the author quotes M. Sanson as describing it by a musical notation; in "dumb madness," however, this symptom is of course absent. We may briefly summarise the indications of rabies in the dog by stating that the animal is at first morose, but fidgetty; that it has no dread of water, but on the contrary will greedily swallow it until the spasms of the throat prevent it from doing so; that it has a great desire to bite, and at first attacks inanimate objects, as wood, straw, &c.; that its bark or howl is altered in tone, and that its fury is vented especially on animals of its own species. The mad dog, if not killed, dies from paralysis or asphyxia.

We pass over the account of the symptoms in man, because, although Mr. Fleming's description is very good, the disease is well known and is accurately described in medical books and periodicals, and the diagnosis in the human subject presents but few points of difficulty.

The morbid appearances observed after death in the lower animals are carefully considered, and are also figured by Mr. Fleming, who, however, observes, and we believe justly, that, as in other nervous diseases, the pathological changes observed in the lower animals and in man, when death has been caused by rabies and hydrophobia, are not generally at all in proportion to the severity of the symptoms observed during life; and he goes further when he states that, in many cases, it would be difficult to express an opinion as to the existence or non-existence of the disease in the lifetime of an animal, if the post-mortem appearances were alone to be relied upon. The phenomena which have been observed are chiefly those of congestion, either of the nervous centres, as of the brain and spinal cord, or of the alimentary tract, as of the fauces, the stomach, and the intestines. In man, repeated examinations have been made after death from hydrophobia, and nothing has ever been found which could satisfactorily determine the nature of the disease. The congestion of certain parts and organs, which has been



observed, may, as Mr. Fleming remarks, be a consequence of the terrible disturbance of the nervous centres rather than a cause of this derangement, and the signs of inflammation are too rarely present to warrant the belief that this process has any essential connection with the phenomena. In a note at the end of the book, however, Mr. Fleming quotes some observations made by Dr. Clifford Allbutt at a meeting of the London Pathological Society, in which that physician describes certain pathological conditions of the nervous system after death from hydrophobia, and which he connects with the nervous disturbances observed during life.

The treatment of rabies and hydrophobia is in some respects unsatisfactory, because the disease, either in man or the lower animals, is necessarily fatal. Mr. Fleming does not pretend to have discovered any method of cure, and he condemns the conduct of those persons who, from credulity or ignorance, believe in the efficacy of remedial measures when once the malady has declared itself, and he is still more strong in his condemnation of those who from interested motives vaunt the discovery of specific cures for hydrophobia. But while he admits, as all must do, the incurable nature of the disease, he, nevertheless, deprecates unnecessary alarm in the minds even of those who have been injured by the bites of mad animals, because it does not necessarily follow that hydrophobia will ensue in all such cases. The somewhat prevailing practice of immediately and indiscriminately killing the animal which has inflicted the injury is obviously a foolish and mischievous one, because it is by no means always certain that the brute is really mad, and thus the human victim may be exposed unnecessarily to the torture of apprehension when, perhaps, no cause of fear exists. The advice given on this point by Mr. Fleming is that a dog suspected of or attacked by rabies, or one which has been bitten by a rabid animal, should not at once be killed and buried unless there is reason to suppose *that no person has been wounded by it*, but if a person has been bitten, then the animal should only be killed if the malady is undoubtedly present. If the case is only a suspicious one it is well not to kill the dog immediately, but, to keep it securely confined and to watch it carefully, so as to observe whether rabies really supervenes; the time required for the development of the symptoms is not long, and when they are developed the animal must be killed and buried. If, too, an animal suspected or affected with the disease escapes from its owner, or from any locality, it is the urgent duty of every one to warn the police and cause strict precautionary measures to be observed.

In the case of the human subject the local preservative

treatment, which alone is assuredly efficacious, must be resorted to before the absorption of the virus, and, in order to be efficacious, this treatment must be prompt, and the poison must be removed by suction, squeezing, washing, and cauterisation. If the bitten part is within the reach of the mouth of the individual attacked, he should himself at once suck the wound vigorously, or allow a bystander to perform this duty; and it is to be observed that the danger of so doing is not very great, for, as is well known in the case of certain poisonous agents of animal origin, they are not absorbed by the digestive canal unless there be some abrasion on the mouth or lips. Expression, washing, compression, and cupping, may all be employed, but, useful as these measures are, they ought to be supplemented as soon as possible by cauterisation of the injured parts, either by a powerful heat or by chemical action. Mr. Fleming does not hesitate to recommend the hot iron as undoubtedly the most convenient and perhaps one of the most effective agents that can be employed to destroy the saliva and the tissues tainted by it, and the actual cautery has this further recommendation, that several articles in common domestic use will furnish the necessary instrument. Of the chemical caustics the most efficacious are the strong fluid acids, as the nitric, hydrochloric, and sulphuric; other caustic agents, as nitrate of silver, corrosive sublimate, and chloride of zinc; and strong alkalies, as strong ammonia, and caustic potash and soda. Excision, also, is a very efficacious measure, but the greatest care should be taken that every portion of flesh likely to have been in contact with the saliva be removed. Mr. Fleming gives some very valuable and interesting tables, showing the number of cases where immunity has followed the bites of rabid animals treated by cauterisation, and of cases of death where no such precaution has been taken, and the results, as might be anticipated, are strongly in favour of the adoption of such prophylactic measures.

In conclusion, we can only briefly advert to the suggestions offered by Mr. Fleming as to the general plans of prevention which ought to be adopted by local authorities in reference to hydrophobia. In addition to taking due care of the health of dogs when employed for useful purposes, he recommends the diminution of the number of useless dogs, and he justly urges this diminution in the case of dogs kept by poor people, both on the ground of the propagation of disease by ill-fed and dirty creatures, and also because the consumption of food required for their maintenance represents so much aliment subtracted from the poor people themselves. But, while thus advising the destruction of useless animals, he no less condemns the practice of keeping pleasure dogs by the rich, which also are a source of



danger and an act of mere extravagance, and to lessen the number of such useless pets he proposes the imposition of heavy taxes. On the important subject of *muzzling*, which is somewhat indiscriminately adopted in some places, and altogether neglected in others, there is much to be urged on both sides; but, on the whole, Mr. Fleming is in favour of this measure, although recommending that the muzzle used should be efficient and humane, so that, while it prevents the animal from doing any mischief, it should allow sufficient space for it to breathe freely.

### V.—Surgery of the War of the American Rebellion.<sup>1</sup>

THE two gigantic volumes before us form only the commencement of the medical and surgical history of the protracted and sanguinary conflict officially described as the war of the Rebellion. With the medical volume we have, as reviewers of facts and doctrines, little to do. It consists almost entirely of a vast accumulation of statistics, the detailed observations of disease being reserved for future publication. When we consider how suddenly the war broke out, and how imperfect the organization of both armies must have been at the time of its commencement, we cannot be surprised at finding some uncertainty as to the exact loss sustained by both sides. The Confederate records were to a great extent lost, but such information as could be derived from them has been embodied in these reports.

In the Northern Army alone we find tabulated in this volume nearly six and a half million cases of injury and disease. Nearly 50,000 died of camp diarrhoea, dysentery, and fever, and an equal number of typhus, typhoid, and remittent; 20,000 died of pneumonia; 20,000 were invalided for consumption, and 12,000 for rheumatism. There were over 12,000 cases of smallpox, 20,000 of erysipelas, and when to these we add yellow fever, diphtheria, scurvy, it is appalling to think of the amount of human misery these tables represent. Of over 300,000 deaths known to have resulted directly from the war, only about 93,000 can be attributed to wounds, about 44,000 of which occurred on the field of battle. The proportion of deaths from disease to deaths from wounds was consequently about three to one. This proportion, although apparently large, is not so great as that reached in the Crimean war, and is, we believe, not much in excess of what must always be expected in

<sup>1</sup> *The Medical and Surgical History of the War of the Rebellion, 1861-65.* Prepared under the direction of Surgeon-General J. K. BARNES, U.S.A. Washington, 1870.

any prolonged campaign. Appended to this volume are a large number of reports of medical directors and other documents. Most of them are but of little interest to the general medical reader, consisting chiefly of the personal experiences of the authors. From these, however, we may select one by Mr. J. S. Billings, as it contains a rather more detailed account than the rest of the organization of the medical staff. To each hospital were attached one surgeon in charge, three operating surgeons, each with two or three assistants, one medical officer to provide for food and shelter, and one to act as recorder, the last named being usually assisted by the chaplain, and one or two hospital stewards. Thus organized the field hospitals furnished almost exclusively the primary treatment of the sick and wounded. The position during active service of the medical officers seems to have varied in the different corps. In one they were ordered to be always with their regiments, and within three hundred yards of the line of battle—a position in which, Mr. Billings says, they were about as useless as they could be. The best system seems to have been adopted in the fifth corps by Surgeon Milhaw. The medical officers were collected in groups as near the front as an ambulance could be brought behind each division. The position was protected from fire by a small breastwork, or by the nature of the ground, and a hospital flag hoisted over it; this was called the ambulance picket. Further still to the rear was a reserve of ambulance waggons and stores, well out of the way of fire. Thus but few ambulances were under fire at one time, and yet all were in readiness. The wounded were brought to the ambulance picket by stretcher-bearers, temporary dressings were applied, and the patients at once sent on to the division field hospitals in the rear. If no active operations were going on a regular sick-call was held daily in the advanced depôts, and all seriously sick were at once sent back to the division hospital. The advantage of this plan over that in which the surgeon is actually on the field is evident, inasmuch as he is always to be found; whilst by the latter plan he may get separated from his stores and assistants, and probably also from his patients.

The supplies of medical necessities and food varied much; many reports containing accounts of almost insuperable difficulties in providing the barest necessities of life for the multitudes of sick and wounded, while others speak of abundance, and in some cases even of excess.

At all times the American surgeons recognised the importance of keeping the field hospitals and those in the immediate rear of the army as empty as possible. The wounded, after receiving such attention as was necessary, or in cases requiring



it having undergone primary operations, were removed at the earliest possible time, often within forty-eight hours, to the larger dépôt hospitals. Their removal was accomplished, whenever possible, by rail or water.

Tents were used to a great extent for the hospitals near the front, but in all large engagements it was of course necessary to utilise barns, churches, private houses, and whatever other shelter could be found. The more permanent hospitals were, as a rule, composed of wooden sheds.

As usual the medical staff suffered severely. A long list of 122 of those who were killed or wounded in the performance of their duties is added to the introduction to the surgical volume, with the remark that the "sad mortuary record, proportionally larger than that of any other staff corps, is sufficient to correct the popular fallacy that in time of battle the post of medical officer is one of comparative safety."

Leaving the first medical volume with this brief notice, we will proceed to consider the surgical. The part at present before us contains the report on the wounds of the head, neck, and thorax resulting from over 2000 engagements or battles. Every effort seems to have been made to obtain an accurate and detailed account of every wound received in these innumerable fights. Forms on which to record all the important features of each case were issued to the medical officers, who also were encouraged to send more copious notes, and pathological specimens from cases of special interest, to the Medical Department at Washington. In this matter, while recognising with admiration the zeal and skill of the American surgeons, and the untiring industry and research displayed by Surgeon-General J. K. Barnes in the preparation of this volume, we cannot shut our eyes to the fact that a very considerable part of the information contained in it is valueless from a purely scientific point of view. When we consider the number of surgeons required, the immense strain occasionally imposed upon them, and the difficulty of keeping notes when one is engaged, as Corporal Trim says, "for months together in long and dangerous marches, harassed perhaps in his rear to-day, harassing others to-morrow, resting this night upon his arms, beat up in his shirt the next, benumbed in his joints, perhaps without straw in his tent," it is not to be wondered at that the notes are in a great part brief and devoid of scientific detail. There can be no doubt that from these reasons the statistics in some parts must be looked on with a slight degree of suspicion. Let us take, for instance, the question of the frequency of pyæmia as a cause of death. We know that intermittent fever was very frequent, and it is highly probable that the signs of pyæmia may in many cases

have been wrongly attributed to this disease when the diagnosis was not confirmed by a post-mortem examination. The same may be said of pneumonia, which is recorded as a very frequent cause of death.

We may now proceed to review, as briefly as possible, the enormous mass of clinical records contained in this volume in the order in which they are arranged by the Surgeon-General.

The work commences with a brief record of 282 cases of incised wounds of the scalp, appearing to involve the integuments only. Of these three died from complications, and three from the direct results of the injury. In these latter, probably, the injury was more extensive than was at first supposed. In the treatment of these wounds sutures were not as a rule used, but when they were no evil consequences followed. The editor remarks that "there can be no doubt that exaggerated apprehensions have been entertained with respect to the employment of sutures in wounds of this class," and with this we entirely agree. No special liability to erysipelas was observed in any variety of wounds of the head, whether incised or contused.

Of incised fractures of the cranium from sabre-cuts we have 49 recorded cases, varying in severity from a simple incision in the outer table to a deeply penetrating wound. In these cases the operation of trephining seems to have been but seldom resorted to. Only in three cases was the "formal application of the trephine" made use of, and in only five others were fragments removed immediately after the injury. Of these cases all recovered but one: this success seems to show that the danger from the operation itself is not great. The mortality among those cases which were let alone, or in which fragments were only removed at a later period, when necrosed, was 12 out of 41, one in 3·4 cases. Amongst these fatal cases we find four at least in which extensive "splintering of the vitreous plate" is said to have been found after death, and in seven more no post-mortem examination was made, but in all probability a similar condition existed. It seems not improbable, therefore, that if the rule laid down by some surgeons to trephine at once in all penetrating incised or punctured fractures of the skull had been followed in these cases, the mortality might possibly have been still further reduced. The fracture is in such cases already compound; splintered fragments, almost certain to necrose, are lying in contact with the dura mater, the exit for the pus which must necessarily form is very insufficient, so that we seem here to have a combination of circumstances which can but be improved by the operation. In some few cases a flap of bone was raised and remained adherent to the scalp. The line of treatment to be adopted in such a case is discussed at some



length; and the conclusion arrived at is, that when the flap includes both tables of the skull, it may with safety and often with advantage be left in the hope of its uniting. When it includes only the outer table it is better removed, as it is not required, the cranial cavity not being opened, and may give rise to troublesome consequences. Of punctured fractures of the skull we have only six recorded, five by the bayonet and one by the sword. No primary operation was performed in any of these cases but one, and he recovered. In this case the patient received a bayonet wound in the left parietal region, and a few fragments were removed at the time of the injury. The patient suffered from right hemiplegia and "aphonia," and on recovery was left weak on the right side, and stammering in his speech.

We now come to a different class of cases, viz., injuries of the head due to causes common to civil and military life. Under this heading we have no less than 508 cases. These are extremely interesting, not so much in their pathology or treatment as in their after-history. The great majority of them have been kept in sight, and at varying times, from one year to four, have been examined by the medical inspector, with a view to ascertain their claims to pensions. Of these 508 cases 331 were merely contusions or lacerations of the integuments without injury to the skull or its contents, and of these every one recovered; 12 were examples of injury affecting the brain, but without fracture of the skull; and of these 14 died, and no less than 53 were discharged for disability. One hundred and five cases are returned as suffering from fracture of the skull, and of these 57 died. With regard to the second class of cases, Surgeon Barnes says, none of the fatal cases "throw any light on the functional or textural alterations of the brain resulting from shock, but leave the subject, which has perplexed pathologists for so many centuries, as inscrutable as ever." The reason of this seems to be, that in the majority of cases no post-mortem examination was made; and one in which it is stated that no fracture or extravasation was found, the cervical vertebræ and spinal cord were not examined, though the patient was killed by a fall on his head from a horse. Of the 53 discharged for disability, vertigo, dizziness, impaired intellect, loss of one special sense, &c., were amongst the more prominent symptoms.

Of the 105 fractures, as before said, 57 died, 19 recovered completely, and 29 partially. Among these last were 3 cases of epilepsy, 3 of hemiplegia 2 of impaired intellect, 2 of deafness, 1 of imperfect vision, and 5 of vertigo and cephalalgia on exposure to the sun. These statistics are interesting, as we do not remember to have seen any such before; and from the

difficulty of following up the after-history of cases in civil practice the like would be almost impossible to obtain.

The next section includes gunshot wounds of the head. Of these 7739 are classified under the head of scalp wounds.

The records under this head are not nearly so satisfactory as those in the preceding chapter. The mortality amounted to no less than 162. Of these only 5 are reported as dying of pyæmia, and in the only one of these fully reported the source of the pyæmia was evidently a bruise of the bone: 22 are said to have perished from inflammation of the brain or its membranes, without any primary lesion of the skull. On carefully reading the brief notes of these cases furnished by the surgeons in charge, we feel obliged to reject the whole evidence as fabulous. Only one post-mortem is recorded, and no mention whatever is made of any examination of the bone. In 54 we have no information as to the cause of death, so that in this most important and interesting class of injuries we cannot say that these reports have added in any way to our knowledge.

The next group of cases, however, somewhat compensates for the want of interest in the last. It consists of 328 gunshot wounds in which bruising of the bone was distinctly recognised at the time. These are well recorded, and it would be interesting briefly to review the complications which were observed. Exfoliation was of course a frequent occurrence, but it seldom affected more than the outer table. Among the whole number we have only one case of pyæmia recorded. A remarkably large proportion of the survivors were more or less disabled. Persistent headache was not an infrequent consequence; 9 cases became epileptic, 10 became insane, 2 aphasic, 14 suffered from impaired vision, 61 from impaired hearing, and many more from vertigo, dizziness, &c. In fact, the results may be summed up thus: of the 328 cases 17 per cent. died, 30 per cent. were disabled from causes directly referable to the head injury, and 53 per cent. recovered. As to treatment, the cases seem to have been left as far as possible to nature. Formal trephining in the expectation of finding pus was performed on twelve occasions only. In 6 pus was found, in 4 between the dura mater and bone, in 1 beneath the dura mater, and in 1 in the brain substance. All the cases terminated fatally; but in no case did the fatal termination seem to depend on the operation.

One hundred and thirty-eight cases of gunshot injury are recorded as examples of fracture of the outer table of the skull only, but after carefully examining the evidence, the Surgeon-General concludes as follows:—"I am disinclined to admit that



the outer table of the skull is ever fractured in the adult without injury to the inner table, either by projectiles of war or any other external violence, except in rare instances of blows, or the impact of missiles upon the superciliary ridge, or mastoid or zygomatic processes, and possibly the occipital protuberance, or by grooving by a sharp shell fragment." In this he differs from Pott, Sir Astley Cooper and Brodie, but agrees with Velpeau and Samuel Cooper. We are quite in accord with Surgeon-General Barnes when he doubts the evidence of these cases, but when we consider that these 138 cases have been recorded by a large number of surgeons, it seems to justify a somewhat shaken confidence in the scientific accuracy of these reports.

On the other hand, we have twenty cases of fracture of the inner table alone so carefully and accurately recorded, and some of them also illustrated, that there can be no doubt as to their correctness. Only one of these cases recovered, the diagnosis being made from a large sequestrum which separated in which the external table was intact, while the internal presented a depressed fracture about three quarters of an inch in length. The diagnosis of such an injury seems from its very nature to be impossible, and its frequency can be merely a matter of conjecture, as except in such rare cases as that above mentioned it is only found when leading to fatal complications. All the cases mentioned here died of cerebral complications, usually the formation of an abscess beneath the injured bone—in two cases complicated with pyæmia; in four trephining was unsuccessfully performed for signs of compression. Stromeyer professes to be able to ascertain the amount of injury to the inner table, in some rare cases, by the cracked pot sound emitted on percussion with a silver probe, but he considers the usual impossibility of diagnosis lucky for the patient, as he "thereby escapes the danger of being trepanned." To the question of trepanning we shall return again at the end of injuries of the head. Fissured fractures without depression were rare from gunshot wounds. Fractures of both tables with depression were more frequent especially in young subjects, and in these parts of the skull well supplied with diploë. Although in all cases there was somewhat extensive splintering of the inner table primary trephining was but rarely resorted to. When possible the fragments were elevated or removed from the original wound. Again, in this class of injuries, as in the former, we have a long list of complications following on the cases of reported recovery. Out of 364 cases only 42 were able to return to duty, 15 became epileptic, and of these 7 had received wounds in the frontal region. Many more suffered from impaired vision, deafness, and the various symptoms before mentioned. Pene-

trating fractures are of course in the majority of cases immediately fatal; 486, however, survived sufficiently long to come under treatment, and of these 84 recovered. Brief notes are here given of nineteen cases in which the patient actually recovered with the ball still in his head; the recovery was in no case absolutely perfect. In some a fistulous track was left in the site of the original wound. The wounds were mostly in the frontal region, but two were in the occipital and several in the parietal. Successful removal of the ball was still more rare, only eleven cases being recorded, so that it would seem that when a ball is well lodged in the head it is better left where it is. Seventy-six cases of actual perforation of the head lived long enough to come under treatment, and of these 17 recovered, showing a percentage slightly more favorable than in the former class of cases. Some of these cases were remarkable illustrations of the saying that no injury of the head is so severe as to be beyond hope of cure. One patient recovered after a ball had passed through from one temple to the other, as likewise did another in whom the ball "entered the cranium one inch above the mastoid process, and presented itself on the opposite side, where the cerebral matter left no doubt as to the fact that the brain was injured."

Having thus reviewed the chief varieties of head injuries, the editor finishes the section on gunshot wounds of the head by discussing the following important subjects:

1st. Removal of fragments after gunshot fracture.

2nd. Trephining after gunshot fractures.

3rd. Hernia cerebri and its treatment.

4th. Contrecoup.

The informal operations of removal of splinters and elevation were performed on 454 cases, with 176 deaths, or 39 per cent. These numbers include both primary and secondary operations, and although each individual case is recorded, the information concerning them is so scanty that it is impossible to come to any very definite conclusions from such data. It seems, however, evident that the removal of loose spicula, and elevation of depressed fragments, can only put the patient in a better condition, and the editor seems thoroughly to approve of the treatment. In 196 cases the trephine was formally applied, and of these 110 died, or 56 per cent., or 17 per cent. more than after the operations of simple removal of fragments or elevation. Of the operations, 46 were primary, with a death rate of 69·6 per cent., 99 intermediary, with 56·6 per cent., and 17 secondary, with 23·5 per cent. Although the editor does not enter into the question very fully, there seems to have been a general objection to the operation of trephining except as a last re-



source. Now, it seems quite clear that in the 46 primary cases, in all or almost all of which there existed a compound comminuted and depressed fracture, which could hardly be repaired without suppuration, the danger to the patient could not have been much increased by the simple removal of a disc of bone, as in the operation of trephining. On the other hand, by providing a free exit for discharges, and removing one source of irritation, the patient's state ought to be improved. The primary operations have, moreover, to bear the blame of all those cases in which injury to the brain rendered life impossible. It is but natural therefore that statistics should show them more fatal than the secondary or intermediary. The question may also be asked whether many of those unsuccessfully trephined at a later period would not have recovered if the operation had been performed at once. We cannot help thinking that the operation of trephining is saddled with a great deal more blame than properly belongs to it. Its statistics are doubtless extremely bad, but when it is done merely as a last resource in intra-cranial suppuration and such-like cases what else can we expect? The older surgeons, who doubtless trepanned in a most reckless manner, did not meet with such mortality. In fact, we have little doubt that the operation performed on a healthy man would be accompanied by but little danger. In compound depressed fractures common sense seems to point to the immediate removal of the depressed fragments, either by simple elevation, or, if necessary, by the use of the trephine, and we should feel inclined to follow the dictates of common sense rather than be led away by statistics which from their very nature must be fallacious. What we have said here applies, we believe, also to incised and punctured fractures, and we cannot help thinking that the rule so long prevalent in this country of trephining primarily in all such cases is sound in principle, and, as far as we can learn, successful in practice.

Fifty-one cases of hernia cerebri occurred, of whom forty-four died. There was nothing special in the treatment or nature of these cases.

The Surgeon-General enters somewhat fully into the question of the possibility of the so-called "contrecoup." Only three cases are recorded in which it has even been suspected. In one the fractures were probably produced after death, and in the second, in which the patient survived, the fracture almost certainly fissured from the point struck; in the third there was a fracture of the orbital plates caused by a Deringer bullet, which perforated the occipital bone and lodged in the brain. Professor Longmore has remarked on this case, which has been before published, that the fracture was probably due to a

“transmitted stroke or sudden impulse of the brain substance itself against the thin bony layers constituting the orbital plates.”

This, then, concludes the subject of injuries of the skull and brain. We cannot help thinking a little more might have been made of this vast mass of material if the cases had been sorted and rearranged a little more according to their similarity to each other, and not alphabetically. We should also have been glad to see more of the valuable summaries added by the compiler to certain sections.

We next pass on to gunshot wounds of the eye, of which we have no less than 1190. In 91 of these, in which one eye was destroyed, the remaining eye was impaired or sympathetically affected, but the details of these cases are very scanty. In all wounds seriously damaging the eyes early extirpation was the treatment pursued.

Gunshot fractures of the facial bones form a most important and serious class of cases. If the patient escaped hæmorrhage, primary or secondary, the results were usually favorable, but the frequency of these complications was such as to raise the death-rate to over ten per cent. The treatment of such hæmorrhage has been much discussed, and in most works of surgery it is allowed that in deep bleeding from the branches of the carotid it is justifiable to ligature the common carotid in preference to attempting a difficult and dangerous dissection in search of the bleeding point. We consequently find that the operation was performed for this class of injuries no less than fifty-four times. The result of one case is uncertain, of the rest 15 recovered, and 38 died. The vessels most frequently giving rise to such hæmorrhage as to require the operation were the deep branches of the internal maxillary. In no case is it recorded that an attempt was first made to reach the bleeding point. When the ligature was not applied the usual treatment consisted of plugging the wound, and the employment of cold and styptics. In wounds of the neck we find hæmorrhage again the great source of danger if the patient survived the immediate injury. Actual wounds of the great vessels themselves were rare, but occasionally they were so bruised by the missile that their coats sloughed a few days after. In one case the internal jugular vein was completely severed by a conical bullet; after the loss of about two quarts of blood the hæmorrhage ceased, but the patient died with symptoms of pyæmia on the sixth day. For wounds of the neck we find twenty-one ligatures of the carotid were performed, of whom 20 died. The hæmorrhage recurred after the operation eight times, and in two of these the other carotid was tied, in one case three, and in the other four days after the



first operation; each patient survived five days, and died of the effects of the hæmorrhage. In the majority of cases death resulted from "exhaustion."

We have therefore recorded here seventy-five ligatures of the carotid, including three double operations, with a mortality of 78 per cent., and this, the editor thinks, will furnish an additional argument against the performance of this operation for traumatic cases, unless the injury involve the main trunk itself, and a ligature can be placed above and below the point of injury.

"If," says he, "the indolent or timid surgeon who, to control bleeding from minor branches of the carotid, prefers to stuff the wound with styptics, or to perform the easy operation of tying the common trunk rather than to seek in the difficult anatomy of the maxillary and thyroid regions to place double ligatures at the bleeding points, he may associate his name with the necrology of ligations; but if his patient recover, it will be generally found to be under circumstances in which the surgeon's intervention was uncalled for."

It is somewhat hard to call a surgeon "timid or indolent" because, perhaps, in an imperfect light, and probably with the haziest possible ideas of the anatomy of the deep vessels and nerves of the neck, he hesitates to undertake a deep and tedious dissection in a sloughing wound filled with blood, and surrounded on all sides by structures, an injury to which might be fatal to the patient. At the same time he finds that the bleeding from which his patient is rapidly sinking is controlled at once by pressure on one carotid. What is more natural than to ligature that vessel? We find, moreover, that in fifty-five of the seventy-five cases in which it was done the bleeding did not recur. We think therefore that the rule of practice to tie always at the bleeding point cannot be absolutely adhered to in such cases, and that, in really deep bleeding from a sloughy wound of the face or neck, the surgeon consults his patient's interests as well as his own in not attempting an extensive dissection to find the bleeding point.

Take, for example, a case we find recorded under injuries of the spine. The patient was wounded by a conoidal ball, which entered the right side of the neck just below the ear and lodged. On the tenth day secondary hæmorrhage occurred, which the post-mortem examination showed to have come from the internal carotid, which was nearly divided "at the point where it enters the skull." The right common carotid artery was tied, and the patient died the following day without any return of the bleeding. The editor here regrets that an attempt was not made to apply a double ligature at the bleeding point. We

cannot conceive any surgeon, however bold or active, undertaking such an operation if he had a full knowledge of the anatomy of the parts.

But few cases of plastic operations for the repair of deformities resulting from gunshot wounds of the face are recorded. The loss of substance after such injuries is, as a rule, too great to admit of successful operation.

The next chapter treats of injuries of the spine.

Seventy-five cases are returned under the headings concussion, contusion and sprain of the spine arising from blows from blunt instruments, falls from horses, railway accidents, &c., but the records are so brief as to be of no value. We find, however, that, of the 75, only 44 were able to return to duty after varying periods of treatment, in some cases extending over many months, 2 died of intercurrent diseases unconnected with the injury, 2 deserted, and 27 were discharged as unfit for service. It is greatly to be regretted that these cases have not been followed out, as we might have obtained from them some valuable information with regard to the remote effects of such injuries.

The operation of "trephining the spine" was not had recourse to in any case either of gunshot wound or of simple fracture of the vertebræ. In only twenty-four cases is any operative interference recorded, and in these it was limited to removal of sequestra or loose fragments. The success of these operations varied of course with the nature of the injury. When the spinous process alone was injured the danger was very slight. In any case the opinion of the American surgeons seems to have been that it was safer to adopt a merely expectant treatment. Taking the whole 642 cases of gunshot wound of the spine recorded here together, the mortality was only 55·5 per cent. There are no statistics to show the mortality in the more severe wounds, but it appears that almost every case in which the cord or its membranes was implicated terminated fatally.

The fifth and last chapter of this volume is devoted to wounds and injuries of the chest. Of the many thousand wounds recorded under this head, only 29 were from the bayonet. We are so accustomed to reading in accounts of battles how some regiment gallantly carried the enemies' position at point of the bayonet, that it is perfectly surprising to find how seldom this weapon is really made use of in actual warfare. Out of this small number of 29, three were almost certainly inflicted on wounded men, one on a prisoner, and one by a refractory prisoner. Of the 29 cases 9 died, 3 in consequence of lesions of the large vessels of the thorax, and the rest from inflammation of the lungs. All the wounds, except 9, penetrated the cavity of the thorax. Under the head of gunshot wounds



of the chest are included part of those of the clavicle and scapula. The total number amounted to over 20,000, and of these 11,500 did not penetrate the cavity of the chest. Among these latter the mortality was only 1 per cent., while in penetrating wounds it was 62·5 per cent. Many cases are recorded in which the missile penetrated the chest without injuring any bony structure, and these were found to be more favorable as being uncomplicated by the splinters of the fractured bone. Wounds of the heart were of course usually fatal on the field, but a few survived long enough to come under observation. In one the ball was found to have penetrated the right auricle, yet the patient lived *fourteen days*, and then died suddenly. Another lived one hour and a quarter after perforation of the right auricle and the left ventricle by a pistol ball. A third lived forty-six hours after perforation of the left ventricle and auricle. In this case the patient was conscious though extremely prostrated. Wounds implicating the pericardium only were less frequent, and according to these reports were invariably followed by pericarditis which in the majority of cases ultimately proved fatal. Three cases of recovery are, however, described in detail.

Wounds of the lungs and their complications of course take up the greater part of this section. Wounds of both lungs were infrequent; fifty-eight cases are said to have occurred, of which nine recovered. The editor seems inclined to doubt the accuracy of this return, believing that many may be cases in which two balls struck the patient at the same time. This explanation seems to be forced and unnecessary, as a simultaneous wound of both lungs need not be immediately fatal if one lung only has adhesions to the pleura. The general treatment of penetrating wounds of the chest was as simple as it could possibly be. If no severe complication was present or no urgent symptoms, a simple wet dressing was applied to the wound and the patient left as much to nature as possible. The wound was carefully examined with the finger when the patient was first seen, and any fragments of clothing or bone within easy reach and the missile itself when practicable were removed. A plan of treatment was suggested by Dr. Howard, which was tried to some extent, and is discussed at great length in this volume. It consisted of "hermetically sealing" the wound at the first dressing. The plan adopted was to pare the contused edges of the external wound or wounds, and to bring them together with harelip pins or silver sutures, and then cover the whole with collodion. If effusion occurred into the chest it was relieved by tapping. A considerable number of cases are reported in which the treatment was tried, but the details are so meagre that it is difficult to come to any conclusion. Many cases were

not operated on till the wounds had begun to suppurate, and then of course it could only do harm, and others were hopeless cases under any treatment. As far as we can see, the operation did but little harm. One or two cases made most marvellously rapid recoveries, but in the majority the wound refused to heal, and the patient was left in much the same state as if nothing had been done.

Drainage tubes seem to have been but little used, and the plan of frequently washing out the pleura was seldom adopted. Yet the results seem to have been on the whole fairly good. A mortality of 62 per cent. is not high when we consider the nature of the injury and the circumstances of the patients. In the treatment of wounds of the lung bleeding was entirely abandoned by the surgeons of both armies, both primarily to arrest hæmorrhage and secondarily to prevent inflammation. In hæmothorax the practice adopted varied with the case. At first to arrest the bleeding cold and perfect quiet were employed. This failing, and no bleeding vessel being within reach (as an intercostal), the wound was closed, in the hope that the pressure of the accumulated blood would arrest the hæmorrhage. If the dyspnœa naturally produced by such a process became urgent, the wound was opened again. It is evident no rigid rule can be laid down in such cases. Pneumothorax was a common complication; but, as a rule, secondary only to the collapse of the lung not causing it. It seldom gave rise to trouble except in oblique wounds. Emphysema was scarcely ever seen except in the immediate neighbourhood of the wound. Drugs were not much used or relied on in the treatment of these wounds. Opium was the chief resource. *Veratrum viride* was found of great service in controlling vascular excitement. Antimony and mercury were but little employed. Stimulants were used with great caution at the outset. It seems, therefore, that the American surgeons recognised the fact, that we can do but little in such cases beyond superintending the exit of discharges, and carefully attending to cleanliness; the rest must be left to nature. The only hope of improvement in the treatment of these injuries lies in the use of antiseptics. If some efficient means could be found, easy of application on the field of battle, which would prevent the decomposition of the contents of the wounded pleura, it is probable the dangers and difficulties attending the treatment of these cases would be diminished at least one half.

Wounds of the large vessels in and around the thorax gave rise to frequent complications. Six cases of wound of the internal mammary are recorded. In four styptics and pressure were used with no success, and the patients died. In one the



vessel was tied, how is not said, and in one it was said to have been secured by passing a piece of bandage under it and tying it round the ensiform cartilage—a process which, if we consider the anatomy of the parts, is somewhat incomprehensible, and we need hardly say did not arrest the hæmorrhage. Hæmorrhage from a wounded intercostal was an extremely rare accident—only 15 cases being recorded here in which it formed a serious complication. There can be no doubt that the artery must be torn through in a very large proportion of gunshot fractures of ribs. It is the hæmorrhage and not the wound that is rare. Of these 15 cases the hæmorrhage was primary only in one third. The best means of arresting bleeding from this vessel is a very old question, and the experience derived from these cases does not seem to have added much to our knowledge concerning it: 4 seem to have received no treatment; of these, 2 died of the hæmorrhage, and 2 of hæmothorax followed by pleurisy; in 2 the bleeding was permanently arrested by the use of persulphate of iron, and in 1 by a compress; in 2 the artery and ribs were included in a ligature, and the bleeding successfully arrested; in 2 the surgeon failed to ligature, and the bleeding was finally arrested by pressure and styptics; in 4 only was the artery successfully tied at the bleeding point, and in 3 of these it is mentioned that fragments of bone were removed before the artery was reached. We may therefore say that simple ligature was effected in 1 case only, and of that the details are so meagre that we are not certain some fragments of bone were not removed. It seems that the ligature is hardly to be recommended. The loss of blood during the operation, and the delay it involves, seems to add materially to the danger of the case. The plan of passing a little linen bag between the ribs and plugging it with lint as recommended by Desault seems on the whole to be the safe and easier method.

Wounds of the axillary and clavicular region are included in this section, as in many cases the thoracic walls or cavity were implicated. As the result of these wounds it was necessary in 25 instances to ligature the subclavian artery. Of these only 5 recovered. In 1 of these the operation seemed to be unnecessary, as the real source of the hæmorrhage was found to come from the supra-scapular, which was also tied. In another the operation was performed three months after the wound, on account of an axillary aneurism, but failed entirely to relieve the symptoms.

Among the twenty fatal cases we find two in which the vessel was tied in the first part of its course, one of whom died from shock, and one from secondary hæmorrhage; two in the second

part, both of whom died too early for secondary hæmorrhage, and two in whom the vessel was tied on the first rib, from below the clavicle, both of whom died of secondary hæmorrhage.

In 9 cases circumscribed traumatic aneurism followed wounds of the axillary artery, and in one of these the extraordinary practice was adopted of first ligaturing the subclavian, and then laying open the sac in the axilla, and turning out the clots which led to such a gush of blood that the patient rapidly died.

From the results of these cases it appears that ligature of the subclavian for a wound of the axillary artery or its consequences is by no means a successful operation, and when we consider the freedom of anastomosis about the shoulder, the sloughy state of many of the wounds, and the inherent difficulties and dangers of the operation, it is not surprising that it should be so. On the other hand the thirteen cases of ligature of the axillary, here recorded, present no more favorable results; but if we analyse the cases, all of which proved fatal, the reason of this is apparent. We find in 1 the axillary was tied for a wound of the brachial; in 3 the patient died on the table before the operation was completed (in one case from entrance of air into a vein); in 5, although tied in the wound, only one ligature was applied; in 1 the hæmorrhage came from the *suprascapular*, and in the only 2 in which a double ligature was applied, 1 died in the course of forty-eight hours, and the other on the sixtieth day from causes not stated, but without any return of the hæmorrhage. As far, therefore, as this report goes there is nothing to invalidate the rule of surgery to tie above and below the bleeding point, and although in a former part of this paper we felt bound to differ from the editor of this work in his remarks on ligature of the carotid for deep wounds of the neck and face, yet here we most cordially agree with him.

Gunshot injuries of the bones entering into the wall of the thorax seldom gave rise to the necessity for any definite operation. In a few cases the sharp points of a fractured rib were removed by the chain saw, and in one the whole clavicle was excised, but the majority of surgeons most wisely contented themselves with removing loose fragments at the time of injury or waiting till the injured extremities were separated by the natural processes accompanying necrosis.

We have thus reviewed as briefly as possible the surgery of this war as far as it is presented to us in this volume. The conclusion that must be drawn from it is that military surgeons are recognising clearly their inability to do more than aid nature in the class of cases under consideration, taking care that by unnecessary interference they do not hamper her in her efforts at



cure. To apply the simplest dressings possible, to provide a free exit for discharges, to pay scrupulous attention to cleanliness, and to regulate the diet on general principles, include all that can possibly be done in the majority of cases. The old rules for the treatment of hæmorrhage established by Guthrie have been in every way confirmed, and the abandonment of venesection has been as complete as in civil practice. In conclusion, when we consider how surgeons must have been raked up from all parts of the country without any previous training or experience in the class of injuries they had to treat, we must say that the reports show a remarkably high standard of skill and knowledge, and are highly creditable to the medical profession of America.

## VI.—Microcephalism.<sup>1</sup>

THE publication of the history of a microcephalic child by Dr. von Bischoff affords him an opportunity for reviewing the evolution theory of brain development, and of combating the Vogt school of physiologists. Of the interest and importance of this subject no doubt can be entertained; we have therefore taken occasion, in bringing this pathological essay under review of our readers, to conjoin therewith a notice of a very valuable paper, hitherto somewhat overlooked, by Mr. John Marshall, of University College, which may safely be affirmed to deserve to be regarded as a standard for guidance in the manner and execution of description of all similar cases.

Dr. von Bischoff gives the following history:—Helen Becker, the child of a portfolio maker, was born in June, 1864, at Offenbach, and died in February, 1872. Both her parents were healthy, and no instance of malformation was known to have occurred on either side. Two children, both living and healthy, had been born previously to the birth of Helen. During the pregnancy with her the mother suffered severely from abdominal pains, but the labour was normal. Her mother subsequently bore three children, the last of which was also microcephalic; the others were well made.

Up to the period of her death Helen Becker made no progress intellectually. Pain and pleasure, joy and grief, were the

<sup>1</sup> 1. *Anatomische Beschreibung eines Mikrocephalen 8 jährigen Mädchens.* Von Dr. Th. L. W. v. BISCHOFF.

*Anatomical Description of a Microcephalic Brain of a Female Child.* By Dr. Th. L. W. v. BISCHOFF.

2. *On the Brain of a Bushwoman; and on the Brains of Two Idiots of European Descent.* By JOHN MARSHALL, F.R.S.

almost only psychical expressions that she manifested. She liked being petted and coaxed, and could discern easily enough if she were scolded; she would then begin to cry, and would get into a rage. She made no distinction of persons about her, whether relatives or strangers, and evinced no particular liking for any one person over another. She knew her name, and listened to the voice, even when not looked at, but gave no heed to anything that was said. She had not a trace of memory. She could scream loudly, but could not enunciate a single word; she uttered principally two tones, one palatal, as "eng," the other nasal, as "a." With these two tones she expressed both pain and pleasure. In pain or anger the note was sharp and shrill. She never indicated her wants by any sound. She would snatch at anything bright that was offered to her, particularly toys of a red colour, then after twisting them about for a time would throw them away. She could see to the length of the room with the left eye, but did not discern objects distinctly until they were brought close to her. She was blind of the right eye; her left eye was subject to conjunctivitis.

Her hearing was good; the sound of a street organ gave her pleasure, manifested by her raising her head and beating time with her hands, and expressing her delight by a loud shriek. Shrill sounds or the barking of dogs she could not bear, and at these she would cry and stop both ears. If, when shrieking, any one said in a loud voice "Bst," she would stop.

Her olfactory sense was well developed, and she manifested gratification or disgust at bad or good odours, although in her habits she showed but slight appreciation of smells, for when she befouled herself she did not scruple to touch her excrement. Her skin was very sensitive to the touch. She had a dislike to clothes; would not wear shoes. The surface of the body was cold, and the hands and feet blue. The sense of taste was well developed, and she liked savoury food, but showed no craving for meat or drink. She took the food given to her, and signified her liking by smacking her lips and patting her stomach. She preferred meat to bread or vegetables; she did not masticate her food; she liked neither wine, water, nor milk, but coffee only.

The heart's action was feeble; the pulse 132, or, after long rest, 116. The bowels acted once a day, of which occurrence, however, she gave no notice, neither did she in micturition. She had no notion of cleanliness; she would masturbate unless prevented.

She was constantly restless and in purposeless movement; she never learnt to stand or walk; she slept very little, and



during sleep had twitching and jerking movements of the limbs. She suffered much from catarrhal affections. When about six years of age she became the subject of rickets. She died at last from phthisis.

The bones were very brittle, and broke on moving the body. The body was miserably emaciated, and weighed only  $17\frac{3}{4}$  lb., and measured 78.2 centm. (= 30.78 in.).

The weight of the brain and membranes was 219 grms. (=  $7\frac{3}{4}$  oz.); after hardening and removal of its membranes it lost half its weight. The cerebellum, pons, and medulla weighed only 27 grammes. Apparently the brain was healthy; its general conformation approximated it to that of some apes. The particular features of the brain are illustrated by plates.

Von Bischoff describes at considerable length the deviations from the normal state, both internal and external, of the brain, comparing these with the observations of himself and other pathologists, together with the accounts of abnormalities of other parts of the body, which we have not space here to reproduce.

Having fully described this case of microcephalism, the author passes to a criticism of the views of Carl Vogt respecting the genesis and meaning of a malformation. These views have of late attracted considerable attention, and by some have been adduced as evidence of the descent of man from apes. According to Vogt, microcephalism is not the result merely of arrest of development of the brain, but is the result of deterioration from arrest of growth, which, according to circumstances, follows sometimes a direction peculiar to man, sometimes peculiar to the monkey tribes; or, again, he regards it as an atavism or ancestral defect in formation, which, in its essential characters, reverts to the primary stem from which man is developed. It has been objected by Bischoff to this explanation of Vogt's that it is not founded upon the actual examination of a microcephalic brain, but merely of crania or portions of crania, the only material that he had at his command. The attempt was a bold undertaking, and was undeniably carried out with great industry and penetration. Considering the scantiness of his material, Vogt doubtless has manifested considerable subtlety of argument and skill in grouping of details. He had for the basis of his investigations three crania of young persons, of the respective ages of five, ten, and fifteen years:—not one new-born microcephalic. Of the growth of the skull of the infant monkey we know nothing. Bischoff consequently regards the conclusions of Carl Vogt on this question as a structure built upon sand. The author, moreover, disposes of Vogt's views involved in the comparison of the microcephalic brain with the

brains of monkeys, by pointing out that the former is noted to be smaller and stunted in its posterior lobes, while these lobes in the monkey tribes are larger. The attempt, therefore, to draw from microcephalism a proof of the descent of man from monkeys fails—the gap between any existing monkey tribe and man will need first to be filled up. The microcephalic brain of Helen Becker can in no way be regarded as an instance of atavism from our whilom forefathers, much as at first sight it may be thought to resemble the brain of a monkey. It exhibited no trace of disease. The frontal and temporal lobes were relatively large, while the posterior lobes were so much smaller as to permit the cerebellum to be visible.

After reference to the deficiencies of the convolutions and fissures of this microcephalic brain, Bischoff urges that the brain of Helen Becker is unlike that of any known monkey. The irregularity and stunted form of the convolutions, and of several of the internal structures of the brain, corresponding with that of no animal that ever existed, are facts opposed to the inference of a likeness between the two. Moreover, the microcephalic brain bears distinct evidence of the disturbance in growth of the human type, or an arrest of its development at about the third month of its foetal life.

One of the weakest points in Vogt's theory of atavism is the great diversity among themselves found in the internal structure of microcephalic brains and skulls, precluding the supposition that this can be regarded as the result of individual variations of any one type. At no stage of their development can the brains of both the human and the ape series be held to be exactly alike, although there may be a general similitude of plan. It may further be asked, if there be such a reversion to a primitive type in the brain, why there should not be the same in the muscular and osseous system of the same individual.

It will always be a matter of scientific interest to inquire whether in any particular instance we have to deal with a case of deterioration or of reversion to a prior type. This point is thus put by Virchow: an arrest of development is the disturbance of the laws of growth, but atavism is just the fulfilling of a law. The most important criterion for guidance is, as Virchow observes, that the simple arrest of growth never brings the individual form to a standstill, from which point it may be repeated generation after generation, whereas in atavism this may occur. It is, moreover, certain that, in however low a type we may place microcephalics, the general want of physical power and of intelligence is such that life could not be maintained without constant care from others. Bischoff, to sum up the argument,



adds, "The microcephalic brain can never resemble the brain of any monkey, nor can it resemble an ordinary foetal human brain, neither can it be regarded as an instance of atavism."

Such also is the conclusion to be arrived at from the study of the description of two idiots' brains by Mr. John Marshall. One of them was from a female, the other from a male.<sup>1</sup> The female came of a healthy family and died at the age of forty-two years, from phthisis. Her height was five feet, and she was well proportioned.

Her senses were perfect and she was not without memory. She had some knowledge of persons and things, but could not carry on a conversation. She could not count. She never manifested any sexual propensity. She was not passionate, but was susceptible of joy and fear. She could not feed or dress herself; in walking her gait was unsteady and tottering.

The male idiot was a relative of the preceding. No ancestral relative had been known to have exhibited any mental defect, but a second child was likewise idiotic and has since died. We are promised an account of this child's brain by Dr. Langdon Down.

The boy, whose brain is described by Mr. Marshall, had a small head and features resembling those of an Aztec. His limbs were perfectly formed, but he was fed and dressed by others; he was unable to walk or stand. He died at the age of twelve years. At the age of ten he weighed only a little over thirty-three pounds. At the age of eleven he knew persons, but he never manifested any signs of intelligence, emotion, or will, or any power of articulate speech.

The weight of the idiot woman's brain was 7·23 oz., viz. cerebrum, 5·52 oz.; cerebellum, 1·41 oz.; pons and medulla ·3 oz.

In the idiot boy the brain weighed 5·1 oz., viz. cerebrum, 3·51 oz.; cerebellum, 1·35 oz.; pons and medulla, ·24 oz.

The substance of the brain in both cases appeared healthy.

Mr. Marshall gives a very fully detailed description of the cerebral convolutions in the two brains, comparing these with the perfect brain, the brain of the one idiot with the other, and the brains of both with those of the higher apes, pointing out differences which show that these imperfect brains are still human, and being so differ as much as any other bodily defect in man differs from a quadrumanal type.

The idiots' cerebra, Mr. Marshall observes, are not merely diminutive brains possessing every convolution, both primary and secondary, proper to the perfect human cerebrum, each

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<sup>1</sup> These two idiots were also described by Mr. Gore in the 'Anthropological Review,' 1863,

having its natural shape, proportion, and position, though on a diminished scale; but, on the contrary, they are profoundly modified in their convolutional forms, which are not merely smaller in bulk, but are fewer in number, of simpler shape, and different in proportion and position, as compared with those of the perfect cerebrum. Mr. Marshall adds:

“Agreeably to the opinions already expressed by other anatomists in regard to similar examples, the condition of the cerebra in these two idiots is neither the result of atrophy nor of a mere arrest of *growth*, but consists essentially in an imperfect evolution of the cerebral hemispheres or their parts, dependent on an arrest of *development* (*agenesie*, *asthenie-génie*) occurring at some stage or other of their metamorphosis from a simple to a higher form.” (p. 542.)

Again, the conclusions drawn by Mr. Marshall from the facts recorded are:

“First. Instead of the idiots’ cerebra having been uniformly and normally developed up to a certain date (say the latter part of the seventh month), and having been subjected to a general cessation of development, they have experienced an inequality or irregularity of evolution in certain of these parts. Secondly. Whilst all parts have been more or less arrested, the frontal and occipital lobes have suffered more than the temporal and parietal. Thirdly. Whilst both the large ganglia at the base of the cerebrum (those cores or nuclei of the cerebral hemisphere, the corpora striata and optic thalami) have participated in this disturbance of the ordinary course and degree of evolution, the corpora striata having been more especially involved. Fourthly. The original vice of formation, in all probability, affected these two pairs of ganglia primarily; and this entailed, as a necessary consequence, an interrupted, irregular, defective, and perhaps retarded evolution of the convolution of the hemispheres themselves. Fifthly. The primitive starting-point of the future idiotic condition dates from a period far earlier than that at which all further evolution ceases; and, in fact, as regards the optic thalami, and especially the corpora striata, probably from a very early period of development indeed. This conclusion is obviously more acceptable to the physiologist (because more consistent with the radical deficiency in cerebral power manifested by idiots) than the supposition that the idiotic state should be due to a sudden arrest of a previously normal development at some later period of foetal life. Sixthly. The anatomical connection which, by the comparison of these idiots’ brains with healthy foetal brains, have been shown to exist (in human brains, at least) between the development of the corpora striata and the frontal lobes, and the optic thalami and the temporal and parietal lobes, has a considerable general interest, and probably has a physiological significance which may hereafter throw light on the convolutions of those several parts. Lastly. The deficiency in the corpora striata and the associated frontal lobe becomes



particularly interesting when we reflect on the special connection of those ganglia with the anterior or motor columns of the cord and on their probable intimate connection in the execution of voluntary movements, *i. e.* in the mechanical expression by the body of those numerous acts which are the outward exponents of that important psychical faculty commonly designated the "will." Now, it is the inadequate performance or entire abrogation of those acts, whether locomotive, manipulative, or articulate, which constitutes one of the most striking characteristics of the idiotic state." (p. 544.)

## VII.—The Pathology of Apoplexy.<sup>1</sup>

It is notorious that terms and names in the course of time cease to bear their original meaning; and that, as their utility declines, they, like many other things, are discarded by some, but are retained and often misapplied by others, who conservatively cling to the usages of the past, and do not adequately bear in mind that change, as Carlyle says, is "the product of increased resources which the old methods can no longer administer, of new wealth which the old coffers will no longer retain." Hence it comes to pass that the evolution of knowledge is in, through, and from old terms; and as knowledge advances, those terms shrink, so to speak, and become, not the names of things, but of parts only of the things they originally represented. In their downward course to oblivion, however, they sometimes cause not a little confusion, and seem determined, like "Puck," to lead persons "about a round." We are led to make these remarks with reference to the term apoplexy, which some physicians object to use, whilst others apply it to cerebral hæmorrhage only or mainly, and others again use it in its ancient clinical symptomatic meaning, going much further, however, than the ancients, and speaking of serous apoplexy, of hæmorrhagic, nervous, embolic, gouty and rheumatic apoplexy.

When we look into the life, so to speak, of this term apoplexy, we are reminded that, at the outset of physical science, objects were first classed according to their outward

<sup>1</sup> 1. *Cerebral Hæmorrhage and Apoplexy.* By J. HUGHLINGS JACKSON, M.D., F.R.C.P. ('Reynolds's System of Medicine,' vol. ii, 2nd edition.)

2. *A Treatise on Apoplexy, &c.* By JOHN H. LIDELL, A.M., M.D.

3. *Congestion of the Brain.* By J. RUSSELL REYNOLDS, M.D., F.R.S., and H. CHARLTON BASTIAN, M.D., F.R.S. ('Reynolds's System of Medicine,' vol. ii, 2nd edition.)

4. *Encéphale.—Congestion.* Par JACCOUD et HALLOPEAU. ('Dictionnaire de Médecine, &c.,' tome xiii.)

5. *Hyperæmia of the Brain and its Membranes.* By Dr. F. VON NIEMEYER, M.D. Translated by Drs. HUMPHREYS and HACKLEY.

condition and phenomena; for instance, animals were classed according to their external appearances and habits; afterwards their internal organs were examined, and the knowledge thus gained necessitated a new classification; at a later period, not only their organs, but the component parts or tissues of their organs were studied, and their classification underwent further change. The history of medicine also teaches that disease was first known by its external features only; as the terms apoplexy, ascites, amaurosis, jaundice, phthisis, tabes, porrigo, &c., show; afterwards post-mortem examinations indicated that ascites might be caused by disease in the liver, heart, or kidneys; jaundice by disease in the liver or bile-ducts; phthisis by disease in the lungs; and, as is well known from more recent investigations, there is in phthisis not simply one, but several kinds of histological change in lung, and in ascites more than one morbid change in the tissues of the liver or heart, and so on.

To return, however, to apoplexy. The old physicians knew it by its external features only. Aretæus and others observed that persons suddenly fell down, were deprived of feeling, of understanding, and of voluntary motion. These phenomena, they thought, constituted a disease, and to it they assigned the name "apoplexy." Post-mortem researches, however, afterwards revealed that the brain was diseased in some cases of apoplexy; and later, as Jaccoud and Hallopeau say, "*Les travaux de Wepffer, et surtout ceux de F. Hoffman, dirigèrent plus particulièrement l'attention sur les empanchements sanguines du cerveau, et firent prendre le mot *apoplexie* comme synonyme d'hémorrhagie cérébrale.*" And he adds: "*Ce fut là le point de départ d'une perpétuelle confusion.*"

It is now known that the symptomatic disturbance designated "apoplexy" occurs not only with cerebral hæmorrhage, but also with many other brain diseases. And if apoplectiform seizures are seen more frequently with hæmorrhage, it is because the latter condition is more common in the brain than many other morbid changes.

For years past some physicians have almost ceased to use the term apoplexy. They have not only observed that persons suddenly fall down and lose consciousness in sanguineous apoplexy and in epilepsy, but they have further noticed that the apoplectic phenomena are sometimes associated with convulsions precisely like those of epilepsy, and such apoplectiform attacks are really not distinguishable from many epileptic seizures.

Trousseau, on this point, says, "During the first years of my practice I saw, or thought I saw, a pretty large number of cases of apoplectiform congestion, but for a long time I have not seen any, yet other medical men see as many as before. Let us



therefore inquire on which side the error lies. A man, for instance, with or without premonitory symptoms, falls down suddenly in an apoplectic condition. When picked up he looks stupefied, and for a quarter of an hour, an hour, or perhaps more, he complains of heaviness of the head and mental confusion, and staggers in walking. In such cases the patient is said to have had apoplectiform cerebral congestion. I used to say so like the rest, but I do not now." And he goes on to tell us that the symptomatic disturbance he once termed apoplexy he afterwards spoke of as epilepsy.

The term epilepsy, however, is now restricted, not simply to a symptomatic disturbance, but to a morbid state which has a defined origin and a natural history of its own. And apoplectic attacks attended with convulsions like epilepsy are called by many physicians epileptiform seizures, the term apoplectiform being used to convey the idea that no convulsive movements are present.

Observation has shown that these apoplectiform and epileptiform seizures occur with various morbid changes in the brain. For instance, a man may be suddenly seized with vertigo, fall down, become unconscious, seemingly with complete loss of power in the extremities, and with stertorous, laboured breathing, distended larger veins, turgid face, and blue lips. In this state he may lie for minutes or hours, then die, and the post-mortem examination may reveal a cyst in one hemisphere, seemingly the remains of old hæmorrhagic effusion. Another patient may be going about, seemingly without a single cerebral symptom; he presently falls down suddenly, loses his senses, and is found half an hour or so afterwards paralysed; he remains unconscious, and dies two or three days afterwards, and a post-mortem examination shows a gliomatous tumour in the crura cerebri.

Persons have been known to be sitting talking, or following their employment, apparently free from any head disturbance, when suddenly they have become unconscious, with complete loss of muscular power, and an abscess has been found in the brain. We remember the case of a man who was working in a ship-builder's yard, when suddenly he lost his senses and became motionless. This occurred on a hot summer's day, and the bystanders therefore thought that he was suffering from sunstroke. He died very shortly afterwards, and a syphilitic growth was found on the surface of the brain. Embolism and softening of the brain, also hydatid tumours and strumous masses, have also produced sudden unexpected apoplectiform and epileptiform seizures. Cerebral hæmorrhage, as is well known, even very small blood extravasation, may cause a person to fall suddenly, and become unconscious and motionless.

As far as we know, there is no disease of brain which may not be attended by, or give rise to, apoplectiform or epileptiform seizures. Not only do coarse masses in the brain induce apoplectiform seizures, but also other morbid changes which to the naked eye are much less evident; for instance, sclerosis of brain. Jaccoud and Hallopeau, speaking of cerebral sclerosis, observe, "Dans les plusieurs observations il est survenu dans le cours de la maladie des attaques apoplectiformes, elles ont été généralement de courte durée, et n'ont pas laissé après elles de paralysies persistantes." Also in the morbid condition known as general paralysis of the insane similar seizures occur, and Dr. Lidell shows that apoplexy is common with atrophy of the brain. In his work particulars of thirteen cases are detailed, and Dr. Lidell tells us that in some of them the morbid appearances in the tissues of the brain and its membranes were very striking, and in nearly all of them very significant. The substance of the brain was obviously diseased in almost every instance. It was more or less atrophied in every case but two. In some instances it was much shrunk. In five of them, all intemperate people, the brain-substance was decidedly firmer than natural, or indurated; and in four, mostly aged persons, it was softened. In all or nearly all these cases the blood-vessels of the brain-substance were found to be dilated in consequence of chronic congestion. In several of them this vascular dilatation was very strongly marked, so much so as to give thin slices of the cerebrum a cribriform or sieve-like appearance. Again, there was hyperæmia and œdema of the brain and its membranes in a large majority of the thirteen cases. In some of them hyperæmia largely predominated; in others œdema.

We also have found the brain much atrophied after death by so-termed "simple apoplexy," although in other respects—that is, as far as the naked eye could appreciate—it seemed healthy. In other parts of the body there was simply great venous congestion.

Dr. Lidell also gives particulars of several cases of so-called "serous apoplexy," in which a large quantity of serous fluid under the arachnoid membrane, and a considerable quantity in the ventricles, constituted the distinctive feature. The pia mater also was more or less thickened, and the convolutions of the brain were atrophied.

Pathologists have almost ceased to speak of serous apoplexy; and Dr. Wilks, we believe, expresses their general opinion in saying, "nothing is more common than to see the sub-arachnoid fluid increased in quantity. It often attracts much more attention than it deserves, for, as a rule, it denotes merely a



shrunk and wasted brain. The brain shrinks, the convolutions lose their plumpness and separate, and the space they occupied becomes replaced by fluid."

Dr. Hughlings Jackson on this subject states, "The fluid has been effused very gradually to take up the room vacated by the wasting of the brain. This is seen strikingly in cases in which there is wasting of but one cerebral hemisphere, and where the serum is effused on the wasted side only."

Abercrombie long ago taught that serous effusion in apoplectic cases is not a primary disease, but a termination of simple apoplexy. Not only was there an excess of serous fluid on the surface and in the ventricles of the brain in Dr. Lidell's cases of "serous apoplexy," but there was also, he says, œdema of the brain-substance, for the surface of a section was found "to be pale, moist, shining, and rather exsanguinated in appearance;" and he holds the opinion that this alleged œdema of the brain probably occurred suddenly; that "the serum suddenly escaped into the perivascular connective tissue of the brain-substance, compressed its capillaries, deprived its ganglion-cells and nerve-fibres of fluid, and thereby arrested their function." He moreover supposes it possible that a sudden vaso-motor spasm of the cerebral capillaries ended in producing the apoplectic attack. However, Dr. Lidell does not give any histological or other observations to support these opinions, but simply, to account for the apoplexy, assumes the sudden occurrence of the effusion into the perivascular spaces.

Another well known fact is, that any disease in, or on the surface of the brain may produce an epileptiform seizure. Tubercular meningitis, arachnitis, especially in children, meningeal hæmorrhage, cyst in arachnoid, and syphilitic surface growth especially. Disease in the skull, with or without any marked changes on the surface of the brain; for instance, disease of the petrous or of the mastoid bone, with or without its common intra-cranial suppuration; or, as Mr. Toynbee showed, ear disease without any naked-eye appreciable change in bone or brain, may give rise to, or at all events be attended by, fatal convulsive seizures. In connection with this point we have especially in mind the case of a man who consulted a well-known surgeon on account of an offensive discharge from his nose. It was evident that he had disease of the ethmoid bone. The same day whilst walking along the street he fell and lost his senses. When we saw him he was totally unconscious, with complete paralysis of extremities. A day or two after this the post-mortem examination showed that the ethmoid bone was much diseased, but there was not the least naked-eye appreciable disease in the cerebral membranes or brain substance.

In Dr. Lidell's work we also find details of the so-called "congestive apoplexy," and the morbid appearances observed in his cases corresponded to those described by other observers. The following details of one of these cases well illustrate the morbid appearances seen in the brain and membranes. The case is headed "Congestive apoplexy occurring in a young woman during menstruation. Exciting cause, strong mental emotion; death in four hours." The autopsy was made by Dr. Lidell eleven hours after death, and he says, "On dividing the scalp, blood in more than usual quantity flowed away; while sawing the skull cap off considerable blood ran out, and after it was removed and the dura mater turned back not less than ten or twelve ounces of blood, including that already mentioned, escaped. This blood was poured out from the sinuses of the dura mater and from the vessels of the brain. The skull was thick, the glandulæ Pacchioni larger and more numerous than usual. The visceral arachnoid exhibited some opacity about the vertex. The sub-arachnoid serum was rather more abundant than usual about the vertex, but not elsewhere. The vessels of the pia mater were everywhere gorged with blood. The surface of each section of the brain was smeared over with blood, which, escaping from the divided blood-vessels, or puncta vasculosa, followed the knife. The brain-substance was normal in colour and consistence. The lateral and other ventricles were nearly empty. No clots of blood were found, though careful search was made." The lungs were emphysematous. The heart was somewhat hypertrophied, rather full, and firmly contracted. The mitral valve in both segments alike was thickened by interstitial deposits of semi-cartilaginous consistence. The aortic valves were slightly thickened in the same way. Owing to persistent contraction the cavity of the left ventricle was small and its walls thick; there was false concentric hypertrophy while the organ was really larger than natural. Nothing else abnormal observed. Dr. Lidell says, "There is no doubt that this was purely a case of apoplexy, and that the attack had been brought on by powerful emotions of the mind. The evidences of cerebral congestion which were revealed by the post-mortem examination of this subject were clear and convincing."

The great congestion which Dr. Lidell observed in the above case was doubtless venous, and it is usual in such cases, and in others when death occurs suddenly and the body is not wasted by chronic disease, to find the large vessels, that is to say the veins of the pia mater and brain-substance, very full of blood. Dr. Hughlings Jackson, speaking of simple apoplexy, truly says, "Some of these cases are put down to congestion of brain, but this conclusion is often drawn from distension of the cerebral



veins, which is a very common appearance when patients have died rapidly from any cause."

This venous congestion is, not the cause, but the consequence of death—is rather, we should say, part of the process of dying.

It is notorious, however, that anything which greatly and suddenly obstructs the flow of blood from the head may produce an apoplectic seizure. Probably one of the best illustrations we can give of this is the case mentioned in Abercrombie's work:—"A boy had drawn his neckcloth remarkably tight and was whipping his top, stooping and rising alternately, when after a short time he fell down apoplectic. The neckcloth being unloosed and blood being drawn from the jugular vein, he speedily recovered." There can be little, in fact to our mind no doubt that the tightened neckcloth, in this case, produced passive congestion of brain, and thereby the apoplectic seizure.

The effect of venous congestion of brain may also to some extent be estimated by watching children during paroxysms of whooping-cough. After coughing severely a child may become very livid, lie back, seemingly very much exhausted, depressed, and unable to move; in still more severe paroxysms the child may fall, become absolutely unconscious and motionless; lips and face be exceedingly livid; pulse not to be felt, and respiration almost or actually suspended. In a case of this kind which came under our notice not long ago, in which death occurred seemingly in an apoplectiform state, post-mortem examination showed great venous congestion in the brain and other parts, a little mucus in the bronchi, but no other morbid signs. In other cases, instead of falling senseless and motionless, children with whooping-cough become convulsed during a fit of coughing.

In order to learn something more about cerebral hyperæmia and its influence and mode of action in producing apoplexy, we now turn to Drs. Reynolds' and Bastian's article, headed "Congestion of Brain," page 425 (vol. ii, 'Reynolds's System of Medicine'), where we read that "congestion of brain" may manifest itself by apoplectic or convulsive seizures, or delirium "may be the most marked symptom of congestion of the brain in certain cases."

The apoplectic attacks, Dr. Reynolds says, usually take place during some muscular exertion, such as lifting a heavy weight, or whilst the patient is blowing his nose, coughing, sneezing, may be straining at stool, or stooping to pick up something from the floor. Sometimes it cannot be traced to any one of these. The attack rarely occurs during sleep. Patients, the article rather naïvely says, do "not wake up and find themselves in a state of what is called 'congestive apoplexy.' They are commonly doing their ordinary work, or trying to do

a little more than they are able to accomplish, when the attack supervenes. During the seizure consciousness, sensation, and power of motion seem to be lost, and the patient is said to have an 'apoplectic stroke;' but those faculties, Dr. Reynold remarks, are not altogether lost, or, if they are, it is for a few moments only; the mind is not in complete abeyance. There are indications that the patient knows, although but imperfectly, what is said to him; he makes some attempt to respond to questions and to do what he is asked to do. He starts at a loud and sudden noise, looks around him, and gives signs of annoyance when he is disturbed. There may be or seem to be momentary coma, but soon there are signs of returning consciousness; there is confusion of thought, bewilderment, passing sometimes into delirium, but more often into a heavy sleep. Or the patient may have a more complete apoplectic seizure; "he may fall down, become unconscious; all the limbs when raised fall down; the pulse at the moment of the attack may be suspended at the wrist, and the breathing arrested, but soon the pulse is felt heaving and laboured and respiration become tumultuous; and, again, in a few minutes both pulse and respiration may go on as they did before." There is an exaggeration of the previous vascular fulness of the face, neck, and head; sometimes a bloodshot eye or epistaxis, not unfrequently vomiting, with apparent faintness and a condition of collapse.

The symptoms of a first attack usually abate very quickly; they may last for a few minutes or for several hours, but most commonly they disappear within an hour, and the patient, although languid and perhaps alarmed, may feel better than he has done for some days before. Upon the repetition of seizure, however, the duration of symptoms is prolonged; the recovery of consciousness, sensation, and movement is less complete; drowsiness is more marked, and if there be some awakening it is momentary only, fresh attacks supervene, and leave the patient lower than he was before.

In the convulsive form—

"The paroxysms that occur have the general features of epilepsy, but they differ from the attacks of that disease in their general history and mode of onset. Congestive convulsions may occur at any period of life, but they are most frequently met with at the time of full maturity, or when that stage is passed. There are usually the premonitory signs of congestion, but these may be very slight; there may be no forwarnings, and the patient may be seized during sleep, or while making some unaccustomed effort. When the attack occurs during sleep it is difficult, and sometimes impossible, to say in what manner it commenced; but when it has come on while the patient is awake and friends are about him, it has been



usually observed that much discomfort has preceded it for a few seconds, minutes, or hours. A tight cravat, worn while making some undue exertion, a sudden alarm, or an indigestible meal rapidly swallowed, may be the immediate antecedents. The patient more or less suddenly becomes confused, then apparently half unconscious, makes some unintelligible sounds, turns red and then blue in the face, staggers for support, looks round wildly or imploringly, and then sits down or falls down convulsed, and a paroxysm, epileptiform in character, supervenes. From this he recovers partially, exhibiting great confusion of mind, headache, muscular feebleness, and sometimes partial paralysis of one side or of one limb. The attack is occasionally followed by quasi-maniacal excitement, lasting from half an hour to three or four hours; after which the patient becomes exhausted and falls into a heavy sleep. From this state he may recover, or during sleep a second or third attack of convulsions may come on."

The apoplectic seizures, Dr. Reynolds states, are most common in advanced life, the epileptiform in middle life: but the latter may occur in more advanced age, and then the attacks grow more and more apoplectiform.

Delirium, he says, may be the most marked symptom of congestion of the brain, and this is observed almost exclusively among those who are advanced in age, but not entirely. The delirium may come on suddenly after a fall or fright; if it occur spontaneously it is first observed towards evening. Sometimes it is preceded by "depression of spirits." After some hours or days of taciturnity the patient becomes cheerful or hilarious, often talks loudly, incoherently, but seldom exhibits any violence, and is managed without difficulty. The patient gets out of bed, wanders about, is very busy; has delusions, but not of a fixed character. In the aged there is sometimes "hysterical crying," less frequently great irritability of temper and attempts at violence, but the latter is usually the result of bad management.

We have given these particulars fully, for they are very instructive, and the symptoms Dr. Reynolds describes we likewise have witnessed in not a few cases. The symptoms above enumerated by Dr. Reynolds are commonly seen in cases where there is passive congestion of brain. Patients with such congestion also generally complain of headache and giddiness, varying in frequency and intensity. Little circumstances annoy, which at one time would have had no such effect; wakefulness is sometimes very distressing. When sleep does occur these patients often dream a great deal, and they sometimes tell us that they have been so busy in their sleep that they feel no better for sleeping; sometimes they dream of "horrible things." Not unfrequently such patients say they

are so distressed by the dreams that they dread going to sleep. There may be and generally are some external signs of venous congestion, livid lips, and probably great turgescence of face and distended veins of neck. In other cases, with equally as many signs of venous hyperæmia and dilatation of right heart, there is no great pain in the head, but distressing giddiness on coughing. A medical man not long ago under our care had well-marked mitral regurgitation and dilatation of left ventricle; the right ventricle also was dilated, the small veins of his face were evidently engorged, and his lips slightly blue. Whilst coughing he said he became so giddy that he could not stand, and in some of these attacks he sometimes lost his sight and fell down, and became completely unconscious. Wakefulness, also, was one of his most marked symptoms; and touching this feature Jaccoud and Hallopeau observe that persistent wakefulness is one of the chief symptoms in this form of congestion, although headache is rarely intense.

If we next consider the conditions which produce passive congestion of brain, we find that Dr. Reynolds enumerates among the predisposing causes such physical conformations as impede the return of blood from the head, and accounts heart disease as the most important of these. Dilatation of the right side of the heart, he says, with loss of both power and valvular competency, is commonly found during life and after death in those who succumb to this form of cerebral congestion. These lesions are common in old age, hence age is a predisposing cause of this congestion. The determining causes are to be found in all conditions which entail sudden changes in the circulation, such as exposure to extreme heat or cold, blows on the head, anything which makes it necessary to hold the breath; also violent emotion, overloaded stomach, stooping, and tightness of dress around the neck.

It is not necessary for us here to enumerate all the causative conditions mentioned in the article before us, as our object in this notice is only to consider the pathological changes which chiefly and commonly operate in the production of apoplexy and its allied phenomena. For the present, therefore, we limit our remarks to cases with dilated heart and venous congestion of brain, because of their frequency and necessarily greater interest to the profession.

We would now inquire in what way the causes referred to produce apoplectic and epileptiform seizures, delirium, and other symptoms above detailed; and we shall probably obtain a better insight into the matter if we study the fatal apoplectic seizures first.

In the causation, as described by the author before us, two



conditions are seen to operate to bring about apoplexy. There is cardio-pulmonary disease, which continuously, more or less obstructs the circulation through the venous system; but there is abruptly added some other so-called determining cause, such as coughing, sneezing, or lifting a weight, whereby respiration is suddenly interrupted or further embarrassed, and the right ventricle being consequently additionally and abruptly distended, its contraction is arrested; thereupon consciousness and motion are lost, for brain function, together with all other organic action within the body, ceases when the circulation stops; and if the stoppage continues life ends. Huxley truly says "life has but two legs to stand upon—lungs and heart—for death through the brain is always the effect of the secondary action of the injury to that organ upon the lungs or heart. The function of brain ceases when either respiration or circulation is at an end." On referring to Dr. Reynolds's description it will be seen that those functions are sometimes brought to a standstill at the moment of an apoplectiform attack.

What the precise changes are which stoppage of the circulation produces in the cerebro-spinal organs, we need hardly say, is to a great extent unknown. But the well-known experiments of Stannius, Brown-Séquard, and Schiff, show that when the supply of blood is suddenly cut off nervous activity ceases. And it is evident that as the blood current is arrested in the chest the venous system must be greatly and unduly distended, and the arterial system proportionately emptied. Venous hyperæmia and arterial anæmia necessarily and simultaneously occur together. As the blood accumulates in the venous system, not only are the large cerebral veins greatly distended, but the minute ones also, and, owing to this undue distension, serum probably exudes from the venules, surrounds the brain-cells and nerve-fibres, and impedes their action; at the same time, little arterial blood being sent to the brain, the cells are deprived of oxygenated blood—we would rather say deprived of blood matter and motion. The brain function seemingly, therefore, ceases, because its cells are hampered and blocked on the one side (passive congestion), and not supplied by force and molecules on the other (arterial anæmia).

The apoplectiform seizures, experience shows, may, in the cases in question, occur again and again, and we may probably understand why sudden death does not always occur in these apoplectiform seizures, if we bear in mind that the medulla oblongata is situated close to the arteries at the base of the brain; therefore, when the heart's action is greatly embarrassed, the medulla may receive a supply of blood whilst the circulation in the capillaries of the more distant convolutions is arrested; hence,

whilst consciousness is lost, there remains some sensibility to excitation in the medulla; respiration and circulation consequently can be restored; and it is obvious that if the determining cause continues only a few seconds, the right ventricle,—provided its contractility is not previously almost lost—will probably, immediately the cause subsides, restore the circulation, and the apoplectic seizure thereupon passes off; and it seems this does occur many times in such cases, especially when the apoplectiform seizures are caused by coughing.

It is more difficult to understand the way in which epileptiform attacks are produced in cases of passive congestion of brain, but it would appear that they are chiefly caused by the brain being largely and more or less suddenly deprived of blood. It is well known that profuse hæmorrhage may cause convulsions. Moreover, Küssmaul and Tenner have shown that loss of consciousness and general convulsions may be induced by obstructing all the arteries supplying the brain with blood. And it is well known that if a bubble of air entering a vein of the neck pass down to the heart, and obstruct one of its valvular orifices, the general circulation is stopped, and immediately loss of consciousness and convulsions follow. And we have also to notice that an epileptiform seizure differs from an apoplectic attack in one feature only, that is, the convulsive movements—in apoplexy movement is entirely lost, whilst in the other it is disorderly. The difference would seem, therefore, to be one of degree mainly; consequently it is conceivable that, if the heart totally fails to send on the blood, there is necessarily an entire suspension of brain action—apoplectic stillness. But if the heart's power be very greatly diminished—almost lost—the circulation may still feebly flow through the more central and other parts of the brain in the vicinity of the large arteries at the base, but fail to reach the more distant cells of the surface of the brain; consequently there may be little or no action in the ganglionic cells of the convolutions, and no consciousness, whilst there are disorderly actions in the other portions of the brain and, it may be, of the spinal cord also, to which an imperfect blood supply is distributed; and excitation from without being conveyed by the afferent nerves to these disordered regions, convulsive reflex movements follow. The movements are disorderly because they are no longer co-ordinated by the surface volitional cells and fibres, which cannot act, for, as we have just said, the convolutions are deprived of arterial blood; and they are, moreover, violent because the portions of the brain scantily supplied with blood are hyperæsthetic; and hyperæsthesia we and probably others have witnessed in portions of the body partially deprived of its blood; for instance, where the main artery of a limb is



1874.]

*The Pathology of Apoplexy.*

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blocked by embolus we have seen hyperæsthesia just above the senseless bloodless portion. And observation has shown that hyperæsthesia also occurs by the side of anæsthesia. This is seen where the cord is completely crushed by fractured spine. In such cases there is a line of hyperæsthesia just above the area of anæsthesia. Collateral observation would therefore appear to lend support to the opinion that the brain-substance within the blood-deprived anæsthetic convolutions is probably abnormally sensitive.

The delirium which occurs in cases of passive congestion of the brain appears also to be produced by the failure of circulation. It seems not unreasonable to assume that, when the cardio-pulmonary circulation is less embarrassed, blood may flow through the capillaries of the convolutions sufficient for consciousness, but not in adequate quantity to sustain healthy cell and fibre action, and that the fluctuating blood-flow with its varying pressure may create a confused action in the cells of the convolutions. Excitative impressions are then received by the convolution-cells in the midst of disorderly action; the impress of the form of things is not rightly received, and the ideational cells evidence their disturbance in the shape of delusions. These delusions may be excited by some circumstance outside the patient, or otherwise by some sensation travelling from the stomach, feet, or other part of his body; and, moreover, we have to bear in mind that the ganglionic cells themselves may have undergone some degenerate change which may be largely concerned in the production of these cerebro-mental symptoms. This may explain why delirium more particularly occurs in aged persons.

Generally before the apoplectic, epileptiform, or delirious seizures occur, patients with passive congestion of the brain are from time to time very depressed, or show signs of irritable weakness, and usually, says Niemeyer, the symptoms of irritation precede those of depression, but sometimes the former do not occur, and the depression exists from the outset.

During the depression the patients' movements are slow and sluggish, and their limbs feel to them as "heavy as lead;" there is much inability to execute voluntary acts, together with loss of interest in things, slowness of thought, and a difficulty in rousing them to consciousness. Indeed, this condition may pass into complete unconsciousness and coma.

It is commonly supposed that these symptoms of depression are produced by the greatly distended cerebral veins compressing the brain-substance. But Niemeyer does not think this extra-vascular pressure is the cause, and he says it is much more probable that the symptoms of depression arise because the

requisite supply of arterial oxygenated blood to the nerve-filaments and ganglionic cells of the brain is diminished.

During the period of irritation which, as already said, often precedes the depression, headache is common, and Jaccoud and Niemeyer think this depends on disturbance in the filaments of the trigeminus going to the dura mater. Such patients do not exactly feel, see, and hear more sharply than ordinarily, but they are annoyed by weaker excitations than usually disturb them; light troubles them, or a slight sound; or an insignificant touch excites disagreeable feeling. They experience troublesome noises in the ears and disturbance of sight; they are very restless, disposed to be constantly "tossing about the bed;" sometimes they are very peevish and show much irritability of temper. These symptoms also are supposed to be caused by the unduly distended veins pressing upon and irritating the brain. It is thought that when the distension is less the nerve-elements are irritated only, but that when the pressure is much greater they are paralysed. Niemeyer considers this explanation very suitable as regards the symptoms of irritation. And we also should consider that the venous distension and backward pressure probably play an important part in producing these phenomena of irritation; yet it must be kept in mind that similar symptoms are commonly seen when the blood supply to the brain is diminished, for instance, most typically, as Niemeyer says, after severe metrorrhagia and other extensive losses of blood. Jaccoud and Hallopeau also say anæmia of brain diminishes the vitality of the nerve-elements, whilst from their excitability being simultaneously enfeebled, they become susceptible to excitative influences, which in their normal condition would be too weak to bring into play their activity. To enforce their views, the same physicians note that when the brain is rendered anæmic by ligaturing the arteries of the neck phenomena of excitation and depression occur. With these facts before us, it would seem that the irritable weakness above described is dependent upon the diminished flow of blood through the capillaries of the brain.

Giddiness, also another common symptom accompanying hyperæmia of brain, is, seemingly, a simple hallucination or illusion of sight produced by the fluctuation in blood streams of the retina, maybe of the optic tract also.

Before we leave this portion of our subject we are required to notice another symptom which commonly occurs with cerebral congestion, and is, perhaps, closely related to the delirium—we mean vivid dreams, which occur very commonly when there is cardio-pulmonary disease and great cerebral and other venous obstruction. Patients may be seen propped up in bed, livid, nodding, seemingly dozing, yet their sleep is exceedingly



broken; they wake up abruptly, stare about, and almost immediately lapse into stupor again. These patients often complain bitterly that they dream so much, and sometimes they beg that something may be given to them to make them sleep, for their dreams would seem to be so vivid that they apparently cannot realise that they have been asleep; sometimes they insist they have not slept for weeks. And these troublesome dreams also occur when the left ventricle is dilated and seemingly failing, and before very great venous congestion is set up. Dreams are, perhaps, becoming more and more interesting and instructive to physicians as well as pathologists, for they find that dreams which disturb patients very much are often the product of brain disease, certainly of brain disorder, notably in commencing insanity, in heart disease, and not unfrequently in phthisis and other morbid conditions. In the cases of venous congestion in question it would seem probable that the brain, owing to its obstructed venous flow, is carbonized, so to speak, and that from this cause disturbance in the psychical centres ensues. But it is also to be kept in view that as the brain actions, owing to its failing circulation, are irritably and excitably weak, there is consequently more or less cerebral hyperæsthesia. And may not excitation from within or from without the body, viz. gastric disturbance, &c., or the air moving over the face, the touch of the bed-clothes, and the like, produce faint action in the ganglionic cells of the convolutions, so reviving many former events which have been impressed on the cells, thus bringing back chaotically or otherwise memory, which Maudsley, with much show of reason, says, is "in every nerve-cell; indeed, in every organic element of the body." Wakefulness is another symptom commonly seen in these cases of cerebral congestion. Patients with dilated enfeebled left ventricles are often much distressed by it; either they lie awake a great part of the night, or their sleep is very interrupted; and as the heart's power increases, not only is breathing relieved, but sleep returns. The sleeplessness would therefore seem to be produced by the failure of cerebral circulation. In order to show that such failure does produce wakefulness, we may be permitted to mention that sleeplessness is exceedingly common and a very distressing symptom in "collapse," notably in collapse of cholera. We have observed many times that whilst patients were livid and pulseless they begged for something to make them sleep. The post-mortem examination of such cases showed venous engorgement of pia mater, brain, and other parts, the right heart distended, but the left empty and contracted; there was more or less fulness of the venous system, and anæmia of the arterial system, and in such cases it is notorious that, as the pulse returns and lividity

subsides, patients gradually become drowsy and sleep returns. Wakefulness occurs not only in the collapse of cholera, but also in collapse produced by perforation of the stomach, &c. Niemeyer also mentions sleeplessness as one of the prominent symptoms of anæmia of brain.

The above symptoms of morbid, increased, diminished, or lost excitability of the brain, Niemeyer observes, occur in the most varied groupings in cerebral hyperæmia, and, he might have added, vary very much from day to day. Not unfrequently, as the circulation fails, restlessness, irritability, or excitement, with pain in the head, occasional giddiness, wakefulness, disturbed dreams, appear; afterwards there are symptoms of depression, followed probably by delirium, by muscular twitchings, convulsions, or by apoplectiform seizures; and we have also in some cases observed that when the venous circulation was greatly obstructed there was general depression, psychical, sensorial, and motorial, but afterwards, when the blood flow improved, the depression ceased and was followed by irritability, delirium, and wakefulness.

Before we leave these symptoms of cerebral hyperæmia it may be useful to call attention to the great similarity between the symptoms of cerebral anæmia and cerebral hyperæmia. This similarity is fully recognised. Jaccoud and Hallopeau say the symptoms of cerebral anæmia often present the greatest resemblance to those of cerebral congestion. Niemeyer remarks, "It is often asserted that the symptoms of cerebral hyperæmia are very similar to or exactly identical with those of cerebral anæmia. This is true in regard to congestive hyperæmia (passive congestion) and anæmia." It will be seen that the symptoms produced by these two conditions correspond, and we have only to remember that in cases of very severe hæmorrhage, when cerebral function is failing, there is, first, restlessness, irritability, vertigo, followed by signs of depression; perhaps likewise by delirium, convulsions, or complete loss of sense and motion—syncope. When patients are recovering from syncope, and the circulation is revived, there are, first, symptoms of depression, followed by signs of reaction, in which there may be great restlessness, delirium, intolerance of light and sound, giddiness, rigors, and even convulsions. When, therefore, the cerebral circulation is failing in cases of venous congestion of brain, and in cases of cerebral anæmia produced by hæmorrhage, the symptoms are very similar, and also when the circulation is being restored in these respective morbid conditions the symptoms are very much alike.

Niemeyer proceeds to remark on the question of similarity in phenomena:



“The hypothesis has been advanced that a certain tension of the molecules of the brain is necessary for normal activity, and that an increase or diminution of this tension, by too great or too slight a fulness of the vessels, modifies the excitability of the brain in the same way. I have already said that this is an hypothesis, and I may add that it is difficult for me to believe that in anæmia of the brain the symptoms of irritation depend on an inconsiderable, and those of paralysis on a decided, diminution of the normal pressure of the blood-vessels on the brain.”

To this it might be replied—of course it is simply an hypothesis, but this correspondence in the symptoms of two suppose opposite conditions has to be explained by physical although “vital” principles. Now it is clear that a certain fulness of arteries and capillaries is required, not simply to produce a normal pressure of blood-vessels on brain, but to enable the serous or other constituents of the blood to pass through the capillary walls, and supply molecules and required motion to the brain-cells; and as the blood stream is lessened through the capillaries, and the tension diminished, it would seem only reasonable to suppose that the actions of the brain-cells are correspondently altered, and that when this tension fails they fail also. This stoppage may be caused by the blood flow being obstructed in the lungs, probably in the left auricle, and the veins then become more and more distended, whilst a lessening quantity of blood passes into the left ventricle and into the arterio-capillary system, and at last this ventricle is unsupplied with blood, and it ceases to contract. Or it may happen that the left ventricle, owing to its great dilatation, or to disease in its muscle without dilatation, loses its contractile power, and sends the blood onwards more and more feebly, and, at last, not at all. The effect—cessation of circulation—is the same in both cases. So again, if the blood be withdrawn from the body, as in hæmorrhage, the vessels are equally emptied, and the actions of the brain-cells proportionately altered. It is therefore obvious that there may be venous hyperæmia and coexistent capillary or arterial anæmia; and we can thus understand that the symptoms would be similar if the circulation through the capillaries be arrested, because the blood cannot pass along the veins, or, if it is arrested, because the blood is not forced into the arteries. The symptoms in these delirious, epileptiform, and apoplectiform seizures are seemingly, therefore, caused by the altered, failing, or arrested brain circulation.

If we now return to Dr. Reynolds’s article we are taught that the apoplectic, epileptiform, and other symptoms indicating congestion of brain are preceded by premonitory symptoms. These evidently indicate some chronic, varying, although, on the

whole, progressive failure of brain function. In describing these symptoms he says, "There is diminished intellectual powers; thought becomes confused, memory treacherous; the patient may be irritable, 'put out about little things,' his senses are dull, hearing is defective, sight is dim, there is 'stupid headache,' limbs feel heavy, the general bearing is changed, the step loses its elasticity."

Dr. Reynolds does not tell us in detail what was found during post-mortem examination in the cases he refers to, but the class of cases will readily be recognised, and it will be observed that the cerebral premonitory symptoms he describes are common to different forms of brain disease, but they occur commonly with atrophy of brain, and experience shows that cerebral atrophy and apoplexy are frequently associated.

In cerebral atrophy it has been found that the cells of the convolutions are wasted, the blood-vessels diseased, and the neuroglia altered. There are often atrophic changes with, perhaps also without, morbid growth, more or less widely disseminated through the body in the cases where the brain is atrophied and the heart diseased. And it is well known that sight and hearing commonly fail in such cases, and there may be atrophy of the optic nerves, and also atrophy of the olfactory nerves (Allbutt). This is sufficient to indicate that the brain's means of communicating with the external world may be, and in such cases commonly is, greatly injured.

Man lives not by bread alone, nor the brain by blood only; organs atrophy and degenerate by disuse. Excitation from without is therefore needed. As sense communicators are injured and destroyed—and advancing research is showing more and more that they are—excitation from without is diminished. Again, histological and other anatomical researches are almost every year further revealing that different morbid changes occur in the intracranial blood-vessels which presumably interfere with the nutrition of the brain-cells and lead to their disintegration. There may be fatty, atheromatous, fibroid, or aneurysmal changes in the vessels, causing them to rupture, and blood being extravasated breaks up the brain-structure. It may happen that the blood cannot flow freely through the capillaries because of failure of the right heart's contractibility, or of some impediment, or because of disease in their own walls and consequent engorgement. In fact, Dr. Bastian remarks that microscopic examinations tend to show that such congestions do occur, and that the capillaries, more particularly those of the grey matter, become twisted and varicose, displaying partial dilatations, or real aneurysmal swellings, complicating either a part only of the calibre of the vessels or dilating them in numerous adjoining parts in their whole



extent, so as to constitute "l'état moniliforme" of Laborde. But, he says, a still more certain mark of old congestion is afforded by the presence of blood-pigment in granular and amorphous condition. This, he thinks, results from stases of blood in the vessels. But he moreover says, occasionally an arterial rupture of one of the minute vessels may take place under the increased strain upon its walls in cerebral congestion. This is all the more likely to occur in elderly people, whose vessels have been weakened by fibroid and atheromatous degenerations. In such cases he has not unfrequently found, after careful preparation, evidences of past capillary hæmorrhages in several of the smallest vessels of the same brain. Dr. Bastian, in a foot-note referring to the aneurysms Charcot and Bouchardat, himself, and others, have found on the minute arteries of the brain in cases of cerebral hæmorrhage, further says congestion may have something to do with their formation, as it certainly has to do with their final rupture, leading to effusion of the blood. We may further note that Rokitsansky and others long since pointed out that, with atrophy of the brain, the intracranial veins are varicose. This condition also is presumed to afford some obstacle to the free return of venous blood. Bearing all this in mind, we may, perhaps, obtain some insight into the manner in which the brain slowly and progressively loses its functional activity; how the cells and nerve-fibres atrophy by diminished use, how their blood supply fails, and the cells and fibres are mechanically injured by extravasated blood and serous accumulations. We are not capable of understanding the intimate working of such brains, yet it is not difficult to comprehend that such damaged brains are likely to act irritably and spasmodically. That they fail to receive impressions, and cannot send back responding manifestations any more, and that the wasted cells of the convolutions cannot co-ordinate movements, and at last fail to initiate them. Even although there be comparatively little evidence of marked change to the naked eye in some of these atrophied brains, yet microscopic examinations are revealing more and more that they are very much diseased, and that the changes in the aggregate occupy a large area of the brain, and we arrive at the legitimate conclusion that many of the brain-cells and nerve-fibres must be incapable of performing their functions, that others are capable of discharging them with great difficulty, and others again disorderly.

Whilst the heart's action is perfectly regulated, so as to supply the blood wants of the brain, many of these damaged cells we can conceive are able to work on without much outward evidence of disturbance. As the heart fails, the wonted

supply of blood is curtailed, and then their feeble actions are easily disordered, and "thought becomes confused, memory treacherous," sense dull, walk stumbling; then vertigo, dreams, delirium, spasmodic movements follow, and probably, last of all apoplectic seizures.

Excitation from without may rouse the cardio-pulmonary and cerebro-spinal centres again and again, but as the degenerations advance, and backward congestions increase, at last no excitation can stimulate the cells to further action. We have thus to recognise that, in the cases to which Drs. Reynolds and Bastian refer, there are chronic changes in brain which of themselves tend to bring about apoplexy and death, and that, at last, failing circulation, puts an end to life. And it is easy to conceive that in these diseased brains a comparatively slight but sudden concussion might send a confusing wave through these decaying cells and temporarily arrest their functions; and in this way we can readily understand how a very small but sudden blood effusion may in such cases produce an apoplectic seizure.

Dr. Bastian truly says the conditions are many which operate to bring about cerebral congestion, and in watching the class of cases considered, this fact becomes more and more evident. It is observed that from time to time acute changes are engrafted on the chronic—for instance, bronchitis on emphysema. Frequently also there is in such cases chronic disease of the kidneys, and nephritis is engrafted on the old renal disease, making it occasionally not easy to determine how far the brain disturbance is due to uræmia or to independent acute changes in the brain itself. There is often chronic degeneration in the stomach, and every now and then acute catarrhal irritation supervenes. There is also from time to time intestinal irritation, or perhaps diarrhœa, or else troublesome constipation. Further, there are external conditions producing too much or too little excitation. All these agencies tend still further to embarrass the heart's action, to indirectly cause disturbance in cerebral actions and to induce apoplectiform seizures.

And it may be generally assumed that brain disturbance itself may, and doubtless does, reflect its disorderly actions to the other viscera. Complicated as our representation of these actions and reactions may seem, yet when we undertake the charge of these patients these disturbances are all forcibly brought before us, and it then becomes only too clear that the actions themselves are far more intricate than any physician or reviewer can take full cognizance of.

Dr. Lidell's cases show, and we can by personal experience confirm his statement, that apoplexy occurs in cases where the brain is atrophied, whilst the remainder of the body shows few



or no signs of disease. Dr. Lidell thinks, as we have already said, that the apoplexy in these cases is caused by sudden transudation of serum into the perivascular spaces of the brain-substance, which compresses the capillaries, and thus renders the brain anæmic and incapable of action. He follows Niemeyer in thinking that sudden effusion may thus occur, and we are not in a position to deny the fact; far from it, we think it not improbable, for we have found serous exudations seemingly from distended veins in liver, kidneys, lungs, pericardium, and other parts, commonly in cases of sudden death. But probably most pathologists would consider that the apoplexy, the venous congestions, the minute blood extravasations, and the serous exudations, are all part of a morbid process, and not the disease itself, and that microscopic and other investigations have yet to show what are the primary changes in such brains.

*(To be continued.)*

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#### VIII.—Reports of the Inspectors of Factories for 1873.<sup>1</sup>

THESE reports commend themselves alike to sociologists and physicians. The pre-eminence of this country is that of its manufactures; and the success of manufactures, if, on the one hand, due to freely available capital and to excellence of machinery, is, on the other, owing not less to the skill of the artisans and to their physical well-being. This last-named condition must always constitute a special topic of the Factory Inspectors' Reports; for those officials are especially charged with the administration of laws devised in the interests—educational, moral, and physical—of the manufacturing classes, and they would sadly mistake their functions did they ever lose sight of the sanitary objects pre-eminently contemplated by the Factory Acts. Yet the sanitary and medical machinery of these Acts has not received its due meed of recognition from officials, nor is it sufficiently understood and appreciated by the profession at large. We feel it, therefore, a duty when opportunity offers to call attention to these factory reports.

Those now before us contain numerous notices of circumstances of life and labour obtaining among the workers in manufactories and workshops, which indicate the operation of conditions and moral causes that need to be seriously taken into account by those who direct the machinery of the State, and who are called upon to devise laws of which the so-called

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<sup>1</sup> *Reports of the Inspectors of Factories for the Year ending October, 1873.*

“working-classes” are the special objects. When we read of the crass ignorance of those classes, of their indifference to education, of their evasions of laws designed in their own interests, and of their mischievous trade rules, we cannot but marvel at that precipitate legislation which has potentially lodged the political power of the country in their hands, and placed the possession of intelligence, education, and a stake in the country, at a discount.

In Mr. Inspector Redgrave’s reports we read of “barbarous, semi-civilised men”; of people “ignorant of everything” except of the simple mechanical work required of them; and as well in his reports as in those of his colleague, Mr. Inspector Baker, we are perpetually told of the neglect by the artisans of their children’s education and physical well-being; of their evasions of and direct transgressions against the laws designed to secure the education and the health of their offspring; of their own sensual indulgences and abuse of wages, and of the absurd and wicked rules they bind themselves under as members of trades-unions. Assuredly the picture of the manufacturing classes to be derived from official documents is not flattering or reassuring.

One of the most mischievous rules we have ever read of, to which artisans will bow under the terror of unions, is that referred to as in force among pearl-shell cutters, by which no mechanical appliance is permitted to be used that may obviate the inhalation of the mineral dust by the workers, and so save health and life.

Both inspectors concur in recommending the assimilation of the Workshops and Factories Acts, so that there may be uniformity, not only in the supervising authority, but also in the conditions of labour. At present the two sets of Acts cannot be worked with harmony or fairness; and the divergence between them in respect of their educational clauses, the prescribed limits for labour, and the provisions for ascertaining age and fitness for employment, leads to numerous irregularities, to evasions of the laws, and to much hardship and injustice to employers. It is enough in this place to make these general statements, although ample evidence could be forthcoming from the reports under examination to substantiate their accuracy. Moreover, there have been sufficient indications that the anomalies and injustice of existing legislation have forced themselves on the attention of some influential members of Parliament, and we hope another session will not be allowed to pass by without a successful attempt to remove them, and to produce a consolidated bill dealing with even-handed justice, and with becoming fitness with all occupations



in which associated labour prevails. At the present time Parliament is occupied with the question of further restrictions upon the labour of women and children, by shortening the hours of work and by forbidding the employment of children at the early age now permitted. As far as we can judge, and it is in harmony with our own feelings on the matter, the proposition not to allow children to engage in manufacturing labour until ten years old is generally approved; but the further shortening of the hours of work, particularly in the case of women, meets with great opposition, and is felt to be prejudicial to the manufacturing prosperity of the kingdom, and likewise a proceeding for which no sufficient grounds have been shown.

The proposal emanated specially from Mr. Mundella, who obtained last year a commission, consisting of two medical men, to take evidence on the effects of labour on women and children employed in textile factories. The report drawn up by the commissioners was by no means conclusive on the points Mr. Mundella wanted to make out, and it was very effectively answered by a counter-statement put forth by a committee of millowners. In short, the picture drawn of the physical evils of employment in cotton, silk, and woollen mills was not to be realised, and Mr. Mundella's case may be regarded as not-proven. But even had the nine-hours bill of Mr. Mundella been shown to be a good sanitary measure for textile workers, and one also compatible with the prosperity of cotton, silk, and woollen spinners, there would have been no evidence either of the excellence or necessity of the proposal, or of its effects on trade prosperity in other branches of manufacture, and yet the Act was to be made general, and to affect every manufacturing operation in the kingdom, with the certainty that some manufactures, more or less, must be prejudiced, and that a moiety of women and children, whose hours of work and the nature of the work itself had never been proved to be physically detrimental to them, must suffer in material comforts and in means of subsistence.

In truth, there is much in this demand for curtailing the labour of women and children to justify Mr. Fawcett's opinion that it owes its origin more or less to the spirit and principles of trade-unionism, manifested and devised to keep male labour dear. But, besides the above general considerations, we may adduce the Factory Inspectors' reports as inimical to the change; and their opinion being that of men thoroughly conversant, by reason of their official duties, with the nature and conditions of labour and the general physical state of the manufacturing classes, it assuredly ought to have the greatest weight in the

decision of the question. Mr. Redgrave has argued out the matter in a very fair and moderate spirit. He contends that to alter the hours of work as at present allowed by Act of Parliament, and to now fix "a hard and fast line, would be in practice surrounded with difficulties." And he points out the change that has come over the system of factory labour, so that, instead of the full number of hours being exacted from the employed, as happened in 1850, when the Factory Act fixed the limits of work, numerous relaxations and voluntary reductions of working time have taken place, and have been dictated by the peculiar habits and wants of different localities, and of different classes of operatives. Consequently, he remarks, "any regulation which is to be made compulsory now must be so modified as to adapt it to these varying circumstances"—a condition not readily met by an Act of Parliament.

Mr. Redgrave proceeds to give examples of remissions in the duration of labour in various occupations; and he likewise calls attention to the permissive power given to work from seven to seven during the six winter months, and to the small use made of it, "notwithstanding the advantage it would be to the women and children." The inspector might have carried his arguments much further, and have pointed out that, even had the labour in textile factories been proved as exhausting as it was alleged, yet in almost all other factory operations labour is not so continuously demanded, and not persevered in throughout the legalised hours, but ceases earlier, or is much more interrupted, and its duration left very much in the hands of the operatives themselves, particularly when on piece-work, which is the rule in very many trades. Moreover, he might further have observed upon the numerous holidays taken by the workmen at their pleasure, and consequently on the relaxation from labour afforded thereby to women also and to children.

The upshot of the whole matter seems to be, that there is no real requirement for the intervention of Parliament to shorten the legal hours of labour in factories in the interests of women and children; that no reliable or sufficient evidence is forthcoming that women suffer by the work imposed on them; that the women themselves are not the petitioners for the reduction of the working hours; that from the high rate of wages and various other circumstances, including the independence assumed by operatives and the licence they claim in apportioning the amount and duration of their labour, there have been and are still going on numerous remissions and relaxations from work in which both women and children partake; and, lastly, that in a multitude of manufacturing processes the women have entire control over the time they occupy in their employment,



and in practice do not continue to work for the legal number of hours.

Most of the above points in the question before us are to be found clearly set forth and illustrated by his enormous experience in Mr. Baker's report. This gentleman is most unwilling to disturb the present arrangement of working hours, and declares the cry got up for shortened time to be that of a small party and of a particular trade. Had we entered upon the particulars of the Commissioners' inquiry into the effects of labour in the textile factories, we should have largely used the valuable information presented in Mr. Baker's report touching the grievances which the Commissioners thought to establish. These were (at least with the cotton-workers, with whom the demand for shortened hours especially arose)—“high temperatures, ventilation, dust, badly arranged privies, and, in the weaving department, over-sized yarn.” It is enough here to say that the inspector disposes of them all as matters not inherent in the proposition they were assumed to be the basis of. They arise, as he shows, from natural and preventible causes, and are most of them shared by other manufactures. What unhealthiness attends the textile factories is to be lessened, not by Acts of Parliament curtailing the hours of work, but by attention to sanitary principles; and, in the instance of “over-sized yarn,” to principles of honesty and fair-dealing.

The portion of Mr. Baker's report occupied with a history of the origin and development of factory legislation will be read with great interest. He is evidently and justifiably proud of the part he has taken in it, for he can point to himself as the first to recognise the need of medical men in order to give vitality to the Factory Acts as sanitary measures, and also as the first medical man entrusted with the sanitary periodical supervision of mill-operatives.

He enters on the details in question in order, as he tells us, to prove “that, from the beginning, factory legislation was a sanitary and educational work, to ‘prevent’ (in the language of the first Commissioners) ‘the deterioration of the physical constitution of the workers, deformity, and disease; to ward off the consequences of deficient mental instruction, and to afford opportunities for moral culture,’ whether to children, young persons, or women.” To this statement of the purpose of his historical sketch of factory legislation he appends the expression of his hope that the need—recognised alike by the Medical Commissioners of 1873 and the Committee of Manufactures—of making the Factory Acts still more efficient as sanitary measures than they yet are, will be adequately, and once for all, admitted by Parliament. Some of the sanitary

evils to be found in textile works (say the Commissioners) "admit of entire removal, and all probably of mitigation, were sanitary inspection of factories rendered more efficient than in the present state of the law it is." Blending in pleasing harmony with this sentiment is the suggestion of the master manufacturers, who say, "We suggest the proper course is to make sanitary inspection sufficient to remove thereby the removable evils, to mitigate those capable of mitigation, and not to pass a law (*i.e.* the nine-hours bill) which, if passed, will be permanent, and will operate as a heavy export duty upon productions supplying 120 millions out of the 220 millions of our exports."

Keeping these admissions and recommendations of Commissioners and of manufacturers before him, Mr. Baker finds a most fitting text for some extended remarks upon factory surgeons and their duties. In those officers he rightly enough recognises the instruments necessary both to make the sanitary inspection of factories efficient, and to accomplish the removal and mitigation of evils attaching to manufacturing processes. To this end he proposes certain extensions in the prescribed duties of those officials, but unfortunately he has not worked out a definite scheme, such as it would be necessary to propound before Parliament could take notice of it and act upon it. Moreover, his remarks on the duties now fulfilled by the factory medical officers, and on those others that might be allotted them in order "to make sanitary inspection sufficient," would have come with tenfold force could they have been illustrated by quotations from the returns made by the officers themselves referred to and published in the last annual report issued by the 'Association of Factory Medical Officers.'

Had our object been to demonstrate the present extent of operation of the Factory Acts as sanitary measures, the part played by the medical officers in giving effect to their sanitary requirements, and the part that might be taken by those officers in rendering them efficient, we should have largely used the valuable information contained in the document above alluded to. But our present business is restricted to reviewing the Reports of the Factory Inspectors, and the recognition of this duty as paramount is our reason for not entering on the important subjects just mentioned. Moreover, our space fails, and one or two important topics in the report of Mr. Baker yet remain for notice.

Such a topic is the question of the employment of married women with families, and especially within a certain period after confinement. The employment of such women in factory work, whereby their offspring become neglected and frequently



sacrificed, and they themselves damaged in health and *morale*, is, on all hands, admitted to be an evil. But the remedy for it is not apparent. A law restricting married, child-bearing women from labour cannot be thought of. Even the proposal to treat them as half-timers cannot be entertained, because impracticable; and the only proposition that has obtained favour is that of interdicting women from work for the three months following delivery. The Medical Commissioners of 1873 concur in this plan, and it is one that meets also with the approval of Mr. Baker.

Yet even this scheme, in our apprehension, is fraught with difficulties, and might likewise be attended with much hardship and injustice. To give it shape and efficiency some very minute law-making would be needed, and law of the sort would be irritating and open to perpetual evasions. Female workers in factories frequently change their place of employment, and are often both hired and paid by other operatives, and not by the masters; consequently, the latter have only an indirect control over them, and know often little or nothing about them, especially in large manufacturing towns. Further, there is no existing machinery for getting information about the social and physical condition of women engaged in factories; no recognised means whereby the owner of a factory can make himself acquainted with the fact of the pregnancy of his female operatives, or with that of the occurrence and date of their confinement—that is, so acquainted as to be held accountable for their employment when rendered illegal. For if the restriction be imposed, the responsibility of employing an interdicted woman must in some degree rest on the factory owner. There is certainly the gossip of a workshop, but such a source of information is not available for legal purposes. And we have to remember that, as Mr. Baker himself abundantly shows, there is a large proportion of illegitimate births among females employed in factories; and we know that, in general, their condition is concealed by such mothers until the inevitable catastrophe of delivery, and even this event is kept hidden as far as possible. In the case of premature birth evidence would be still more difficult of attainment, and concealment of delivery more easy. And suppose an inquisitorial machinery devised, who are to undertake the ungracious office of inquiring into the expectancy of a child-bearing woman, or the fact of her confinement within a recent period should she reappear as a worker at the factory? Again, what period of pregnancy will the law fix as that whereat the delivery of a child shall be accounted to disqualify a woman from work? Yet some period must be fixed, as children will at times come into the world

prematurely, and no one could insist upon three months' suspension from work in the case of a woman who has had a miscarriage, or of one who has had an infant still-born or dead soon after birth. Such restriction would scarcely seem necessary, if we rightly apprehend the primary purpose of the rule be to save infant life by securing the nursing and breast-milk of mothers. And without urging other difficulties in the way of making such proposed legislation effectual, we may express a doubt as to the value of the result obtained; for what security would there be, in many cases, for the due nursing of infants by their mothers when unwillingly debarred from their usual labour? There could be none for women determined or driven to work, for such could and would find various occupations not under factory supervision, and probably such as would be more detrimental and exhaustive than those they commonly pursued. The more indigent would be placed in a worse position for nourishing themselves and their infants; the dissipated would find more time to spend in sensual indulgence, and possibly less to give to their offspring than if they continued at their regular employment. And, lastly, if child-bearing involve the penalty of suspension from work, and subject women to annoyances and the irritation of legal supervision and official inquiries, we are, to say the least, not fostering the love of offspring, but rather making the birth of children an evil.

We advance these remarks as precautionary suggestions, not as conclusive arguments against legislation in the direction proposed. To men conversant with the minute details of the Factory Acts, and with the circumstances attending their enforcement, such as are the factory inspectors, many of our difficulties may appear slight; but we have put them forward after mature deliberation, and particularly as we have a dread of too paternal and minute legislation. Indeed, for our part, we hold that laws may overstep the state of society, its degree of education, and its enlightenment. "*Quid leges sine moribus*" is an old aphorism, but never out of date. And we are disposed to look for a remedy of the admitted evil of employing women with child and recent mothers in factory labour to the advancement of education, of intelligence, and of right moral feelings among such people; to the influence of persons of position above them, and more especially to that which every manufacturer can exercise in his own factory, both by counsel and advice, and by special rules.

If Parliament is to be called upon to deal with the evil by distinct legislative enactment, the only practicable scheme that occurs to us is to give permissive power to the owners of factories, to their principal agents, and to the inspectors, to



call upon the factory medical officers to examine and certify to the physical condition of a woman advanced in pregnancy or of one recently delivered as regards her fitness for the labour required of her. In the case of their refusing to certify to their fitness, such women to be relieved from work.

We have dealt so largely with this last question that we must forbear further observations on the very instructive report of Mr. Baker, except so far as to state that he traces much of the improper employment of married women in factory labour to the idleness and dissipation of their husbands.

The present Parliament has, as previously noted, before it a Factory Bill providing for some reduction of the working hours of women and children, and, in the case of the last, advancing the age at which both half-time and whole-time work may be entered upon; and, what is more important still, introducing an educational test for work. To reduce the hours of labour is, as already urged, a questionable proceeding, the more so if it is to be extended to all occupations; but we wish success to the other clauses to regulate the ages for work and to encourage education.

### IX.—Bucknill and Tuke's *Psychological Medicine*.

It seems to be a law with the writers of books that a new edition, when called for, should be not merely revised but also enlarged, often "much enlarged." It is, in short, a law of progressive development dictated by favorable conditions of existence. But however much this law may commend itself to publishers, printers, and authors, it cannot be accepted with entire thankfulness by students. To these last, particularly if medical, the aspect of the volumes thrust upon them in the shape of outlines, principles, elements, and manuals, accounted to furnish them the necessary pabulum for examinations, is assuredly enough to terrify them. On the side of authors the aim is fulness, completeness, and exhaustiveness; on the part of students the result is cramming, repletion without digestion, and exhaustion. The enlargement of a new edition seems always conceived to confer a benefit and to be a 'bid' for farther success; but this view of the matter is a mistaken one. Enlargement is much more readily effected than re-

<sup>1</sup> *A Manual of Psychological Medicine, containing the Lunacy Laws, the Nomenclature, Etiology, Diagnosis, Treatment, &c., of Insanity, with an Appendix of Cases.* By J. C. BUCKNILL, M.D., F.R.S., and DANIEL HACK TUKE, M.D. Third edition. London, 1874.

vision, yet it is this latter process that rightly gives value to the new issue; and revision implies as much the contraction of redundancies and the striking out of what is erroneous as the introduction of what is novel. Of the multitude of books that reach reviewers how many chapters are but *réchauffés* of matter served up in previous editions and previous books scores or even hundreds of times!

These remarks are suggested by some that appeared in the notice published in this Review (October, 1863), of the preceding edition of this joint treatise by Drs. Bucknill and Hack Tuke. We then expressed a hope that the next edition, when it appeared, would be reduced in dimensions "befitting its title of a manual." This desideratum has not been accomplished, and, in fact, the growth of this third edition above the second has far surpassed that of the second above the first; the progressive enlargement in pages being represented by the figures 560, 600, and 800.

Yet the authors deserve credit for acceding to the recommendation for curtailment we then gave, by the omission of the two historical chapters as well as of that of the relations of civilisation to the production of insanity. Nay, they have proceeded farther than we suggested, having cancelled the chapter descriptive of the "Amelioration of the condition of the Insane in modern times, especially in regard to Mechanical Restraint." Here again we cannot but commend them, for although a notice of amelioration accomplished in recent times cannot fail to be deeply interesting alike to the profession and the public, yet reference to it is inevitable when the subject of treatment is dealt with, and an expanded account of it is out of place in a "manual" addressed to medical students or to practitioners seeking to learn primarily the nature and treatment of insanity. Taking the same ground, we could advance a step farther, and suggest the removal or else great curtailment in any future edition of the sections on synonyms and definition, on classification, and on statistics. The history of definitions of insanity represents fruitless labours to define a universal negative by positive terms. No absolute standard of sanity exists; it is a condition relative to the age, to the customs, habits, and beliefs of society, to the history of individuals and the incidents of individual existence. The presumably insane man must be compared with his fellow-men living under like conditions and influences, and with his former self—his actions and emotions, his motives and beliefs.

The classification, again, of insanity, having no pathological basis, so far as most of its forms are concerned, is a subject admitting of as many opinions as there may be writers upon it.



A truly psychological basis is unattainable, because the variety and changeableness of disordered function in various individuals, and in the same individual at various times, give scope for as many different interpretations of the disordered cerebral action as there may be psychologists examining it. Even the joint authors of this volume are not in accord as to the classification to be adopted. Dr. Tuke writes a long chapter, giving a "sketch of the various forms of insanity from a somato-ætiological point of view," and numbering those forms at twenty-two. But when Dr. Bucknill undertakes an account of the forms of insanity, in writing of their diagnosis he recurs to "the well-known classification of insanity into Mania, Monomania, Melancholia, Dementia, and Idiocy," adding,— "not because we think this classification unexceptionable, but because it seems to be a convenient one, founded upon the most prominent phenomena of the disease, to be provisionally used until a more scientific classification, founded upon the pathological causes and real nature of insanity, can be established" (pp. 393-4). But, further, the contentment of Dr. Bucknill's mind with this imperfect differentiation of mental disorders appears to have become disturbed even during the passage of the work through the press, for we find in the appendix yet another attempt at classification of a more ambitious character, wherein the "psychical characters of phenomena are combined with pathogenetic relations and pathological conditions; the first forming the classes, the second the orders and genera, and the third the species."

We congratulate its author upon the ingenuity and analytical skill exhibited in the construction of this last-propounded classification; we do not here discuss it, but remain of the same mind as he himself was when he wrote the chapter on diagnosis—that the old divisions recognised by Esquirol are the best for practical purposes. And we regret that those divisions were not employed to furnish the basis for the descriptions and remarks on the forms of insanity; for the plan, or rather the want of plan, pursued in this otherwise excellent work, coupled as it likewise is with an allotment of different portions to the two authors, has involved much repetition and needless writing. Chapters III, IV, and V particularly exemplify the redundancy and repetition complained of. In Chapter III we have an excellent account of the "Various Forms of Insanity;" in Chapter IV, a "Brief Sketch of the Forms of Insanity from a somato-etiological point of view;" and in Chapter V we are taught to diagnose insanity, or, in other words, are made familiar with the various forms the malady assumes. Then, again, in each of these chapters we

find the forms of insanity treated of under different names by reason of the varied classification adopted. This want of cohesion of matter and of harmony of plan and thought detracts from the clearness and precision the reader desires, and tends to confusion and misconception. In a treatise of this sort, professedly designed as a "manual" to indoctrinate the unlearned and ignorant in the accepted facts and opinions of the day, the principles of composition and construction cannot be too uncomplicated or the nomenclature too simple.

It is, however, a much more cheerful and welcome task to us to note the excellences than the defects of this volume. The former are indeed numerous and far outweigh the latter. What has been done in the way of improving this present edition is of great importance. Thanks to modern research and to the refined and most ingenious modes of modern histological investigation, the pathology of insanity has within the last few years made great progress. The confused and contradictory observations of past but yet very recent times seemed to render an elucidation of the morbid anatomy of mental disorders hopeless. The spiritualistic school alone found consolation in them, and could taunt the materialists with the vanity of all their gropings to discover visible lesions of a wounded spirit. However, although we are yet far from able to associate distinct aberrant psychological manifestations with definite pathological changes, yet we can point to very distinct abnormal changes in brain-structure, and to the concurrence of such with certain phases of mental disturbance. In this new edition the advancement made in our knowledge of the minute pathology of insanity is well represented, the authors having gladly and thoughtfully "availed themselves of the practical knowledge of cerebral histology possessed by one of its most successful investigators, Dr. J. Batty Tuke, of Edinburgh." Our readers have already been made acquainted with the valuable labours of this able observer by the series of original communications he contributed to this 'Review' on "The Morbid Histology of the Brain and Spinal Cord as observed in the Insane," published in the volumes for 1873. In the work before us, in the supplementary chapter on "Morbid Histology," Dr. Batty Tuke has summarised the results he then arrived at, and illustrated them by some excellent coloured plates, which will greatly assist the reader in his attempts to realise the phenomena described.

The general chapter on the "Pathology of Insanity" has evidently been very carefully revised by its writer, Dr. Bucknill, and may be looked upon as probably the most complete essay we have in the English language on the subject. A "supplementary note" is affixed to it by Dr. Hack Tuke, giving Dr.



Lockhart Clarke's description of the several layers distinguishable in the grey lamina, and also the classification of the convolutions of the brain proposed by Gratiolet, and adopted and illustrated by Professor Turner, of Edinburgh. The latter information, with the accompanying woodcuts, is of much utility, as it will enable students of cerebral pathology to describe definitely and clearly the site of lesions they may discover, a matter of vast importance if ever we are to arrive at the facts of the localisation of functions.

Dr. Hack Tuke has appended other "supplementary notes" of much interest. We refer to those on "Sphygmographic Tracings of the Pulse in the Insane," and on the "Hand-writing and Composition of the Insane." Of the latter, the specimens given have been chiefly collected by himself, and are certainly very characteristic of the insane mind. Indeed, the insane will often much more vividly display their mental aberration in writing than in conversation, and in cases where a doubt as to the mental disorder may exist the examination of written matter will at times materially contribute to remove it.

The concluding chapter on treatment is very full, and Dr. Bucknill has taken much pains to collect facts respecting the uses of the drugs most recently experimented with in the medicinal treatment of insanity; and it will be satisfactory to his readers generally to learn that he now writes on treatment with larger and wider views, derived from greater experience with private cases during later years. He says—"In our former editions we have written of treatment perhaps too exclusively from the stand-point which we occupied as the physician of a public asylum. We wish now to fill up our sketch of treatment as it is applicable to patients of the upper classes, who are usually treated in private homes or in private asylums." Advice in reference to such patients would be highly appreciated by the general practitioner, who must be more frequently than not disappointed when looking for instruction in the accepted treatises on lunacy how to deal with a patient who is not a fitting subject, from some cause or other, for removal to an asylum, but has, for a while at least, to be managed in his own home. Unfortunately, however, Dr. Bucknill, although desirous to fill up his sketch of treatment for patients outside asylums, has little to tell the puzzled practitioner. His advice amounts to this—send the patient forthwith to a private asylum, because home treatment that can make an approach to efficiency is enormously costly, and at the best inferior far to the treatment obtainable in a good private asylum. This is small consolation and slender instruction to the medical man who knows he must deal with a patient at his own home, and we could well wish that

Dr. Bucknill had advised him "to make the best of a bad job," so as to render home treatment as satisfactory as practicable. We quite agree with him that it cannot be made equal to that to be got in a well-arranged and managed private establishment for lunatics, particularly in regard to recent and curable cases, and we fully appreciate the value of his advice as to the choice of an asylum; but at the same time experience teaches us that it is a most common event for practitioners to have to treat patients for days and weeks together before removal to an asylum is sanctioned by friends. Indeed, there are occasionally cases of temporary mental derangement which as little require removal to an asylum (with the abiding stigma of certified and accredited lunatics, so abhorred both by patients and friends) as do the victims of delirium tremens. Concerning all such cases the general medical practitioner naturally looks to the teachers and writers on insanity for instruction and guidance, both with regard to their management and medicinal treatment, and for the most part he certainly looks in vain.

On the thorny question of the use of mechanical restraint Dr. Bucknill declines to enter. He holds fast by his formerly expressed opinion and recorded experience, and thus tacitly condemns those who at the present day reject the teachings of Conolly and aim at a reaction of opinion:—"We have" (he writes) "for sixteen years conducted a large asylum, whose admissions during that time have amounted to 2400 cases, without having had occasion to resort to the employment of mechanical restraint in the treatment of insanity. We entertain precisely the same opinions on the employment of restraint and seclusion as those expressed nineteen years ago." He admits the necessity of coercion in surgical cases, and also the occurrence of extraordinary examples of mental disorder in which, too, it may be legitimately employed. His mature conviction is, that "Restraint, except in cases so rare that they may be left out of consideration, is always an unmitigated evil, and seclusion, wisely employed, is frequently an important and valuable remedy."

Our remarks will have exhibited to the reader some of the main features of this voluminous work and likewise our general opinion as to its merits. To express the latter in a few words—we regard this treatise on psychological medicine as by far the most complete in the English language.



X.—On Skin-Grafting.<sup>1</sup>

WE have no doubt that many of our readers are acquainted with M. Ed. About's amusing little story, *Le nez d'un notaire*. A rich and handsome young notary has the misfortune to lose his nose in a duel with a Turk. The doctor who was in attendance was quite prepared, as soon as he had staunched the blood, to sew on the lost part, and thus to repair the injury which the notary had received. But when they searched the spot where the encounter had taken place the piece was nowhere to be found; and an ill-favoured, half-starved cat, which had been noticed prowling about the garden, was more than suspected of having made away with it. What then was to be done? A Paris surgeon of eminence was called in, and he proposed a plastic operation after the "Indian method." But the notary could not bear the idea of carrying a large scar on his handsome forehead for the rest of his days. The "Italian method" was then suggested. But the patient shrank from the pain and inconvenience of a flap taken from his own arm while the limb was kept bound to his head for a month. Could not his wealth procure him something better than this? Could not some one else be found who, on due payment, would allow a flap to be cut from his arm, and who would remain for a month tied to the notary like a Siamese twin? The experiment might at least be tried. Accordingly, a poor water-carrier is called in from the street, and he readily consents to undergo the operation for the sake of the rich reward that is held out to him. The operation is successfully performed, and in due time the notary's nose is restored. But—and herein consists a great part of the fun—the skin taken from the poor water-carrier retains a strange sympathy with the place of its birth, and the notary finds to his dismay that his nose is liable to go through the most unusual changes of size, temperature and condition according to the circumstances in which its original owner is placed, until on the very morning of an auspicious marriage he finds to his horror that it has perished entirely—the water-carrier having met with an accident which necessitated the amputation of the arm from which the flap of skin had been taken. Such is a brief outline of the story. But it is worked out by M. About with many amusing details. It serves, however, to show what train of ideas the transplantation of skin

<sup>1</sup> 1. *Skin-Grafting*. By JOHN WOODMAN, F.R.C.S., Medical Officer to the City Workhouse, Exeter. Second edition. 1873, pp. 31.

2. *Sul Trapianto Cutaneo*. Parole del DOTT. AGGIO CASELLI, Chirurgo primario nell'ospedale di S. Maria Nuova in Reggio-Emilia. Bologna. 1873, pp. 10.

from one person to another has called forth in an unprofessional imaginative mind.

The notion of restoring lost parts by plastic operations was one which suggested itself to surgeons at a very early period. Celsus speaks of the reparation of the ears, the nose, and the lips in a way which is not encouraging, but which we believe expresses with tolerable accuracy all that surgery can even now accomplish. In his chapter upon this subject he says:—"Now, mutilations in these three parts, or any similar, may be cured if they be small; if considerable, they either do not admit of reparation, or are so deformed by it that they were less unseemly before."

If we may judge by the way in which Taliacotius speaks of the transplantation of skin, it would appear that before the publication of his work, in 1597, such operations had been but little thought of or practised. Of his own method he speaks as one which was previously unknown, and he gives minute instructions for carrying it out, as if it were a thing about which surgeons had no experience.

Ever since the sixteenth century operations upon the Taliacotian principle have been occasionally performed; but they have received a great impulse of late years. The introduction of anæsthetics has made operations of expediency more justifiable, and therefore more frequent; and the activity which has recently been shown in all departments of our art has found one of its outlets in this direction. But there is another reason why such operations are more numerous than before, and that is because the recent suggestion of M. Reverdin to graft minute pieces of skin has set surgeons thinking whether larger pieces might not be transplanted in a similar way. Thus we have lately had not only an increase in the number of true Taliacotian operations, but also a variety of experiments standing at intermediate distances between them and M. Reverdin's practice. To these we shall refer presently.

It is not our intention to speak of plastic operations in general—of those, for example, which are so often practised upon the eyelids or in cases of extrusion of the bladder—in which a flap of skin is turned upon itself, but not wholly detached. But we shall confine our remarks to cases of transplantation and skin-grafting, properly so called, in which a piece of the integument, larger or smaller, as the case may be, is entirely separated, and removed from one spot to another.

Several cases have been recorded within the last few years in which a large piece of skin, measuring perhaps three or four inches square, has been transplanted from one part of the body to another by a Taliacotian operation. Thus a flap has been



transferred from one leg to the other, or from the belly to the arm—the base being gradually divided, so that at the end of a fortnight or three weeks the flap was wholly separated from one part, and firmly attached to the other. Such an operation as this, if planned with due regard to the vascular supply and skilfully carried out, will effect the object which the surgeon has in view, and furnish a large piece of integument to fill up the granulating surface. In appropriate cases it may no doubt be resorted to with great advantage. But the cases to which it is applicable are not numerous, and the strict confinement to a particular, and perhaps very irksome, posture which it entails is a great drawback. The operations which are based on M. Reverdin's suggestions are free from these objections; but then they do not attempt to accomplish so much at one stroke. They range over a great extent, and carry the principle of transplantation down almost from the large flaps that we have spoken of to mere cuticular dust. M. Reverdin's proposal was to transplant small fragments the size of a millet seed. This plan Mr. D. Fiddes, of Aberdeen, attempted to improve upon, by suggesting that it was enough to scrape a few particles off the skin, and to apply them to the wound. Others again have sought to transplant pieces half an inch in diameter. Neither of these proposals has met with much success. It seems to be agreed that it is not enough merely to allow the scrapings off the skin to fall upon the raw surface. Probably by this practice only the effete epidermic scales drop, and few or none of those that have any vitality are deposited on the wound. With regard to the transference of larger pieces, in most instances, the transplanted skin has perished, but even where it has lived, and attached itself, it has not shown the same power of generating new skin that the smaller fragments have done. To use a new word, it has given rise to no "cutification." M. Reverdin's suggestion remains, therefore, much on the same ground on which he placed it. Experience has confirmed its value, but not added much to its details. And the practice that appears best to adopt is that of transplanting a number of millet-seed fragments, and placing them about half or three quarters of an inch apart. By this means the utmost amount of benefit which skin-grafting is capable of yielding is obtained with the least chance of disappointment and failure.

If the surgeon determines to try the effect of transferring larger pieces, the question arises whether such pieces should be taken from the patient himself, or from some other individual. Whether they are taken from the one or the other, the practice is alike open to the objection that a sore is made the size of a sixpence, which may itself be a source of trouble and annoyance.

Taking the piece from another patient is liable to the further objection that some infection may be engrafted with the skin, and this objection is so serious that the practice ought never to be carried out without the full assent of the patient upon whom the graft is made. Sometimes the piece has been taken from an amputated limb, but this practice is open to the same objection, and is even more repugnant to the feelings. In the way of experiment it is allowable enough to try these plans, but when it has once been ascertained that they have no special surgical value they ought not to be repeated. It has from time to time been a question among dentists whether teeth could be, and ought to be, transplanted; and we believe the attention which has been given to the whole of this subject lately has led many of them to reconsider the matter, and to make fresh experiments, so as to determine whether teeth can with advantage be transferred from one person to another, and if so under what conditions. That teeth which have (for example) been knocked out by a blow, and speedily replaced, can live and grow, and be serviceable for many years, there is no doubt. Many instances of this replantation are upon record. But whether teeth can be transferred from one part of the dental arch to another, or from the mouth of one individual to another, are very different questions, and upon them the opinions of dentists are much divided. We believe, however, that the subject is engaging the attention of the Odontological Society, and we have no doubt that ere long a practical conclusion will be arrived at.

But to return to M. Reverdin's skin-grafting. It has been a problem how the beneficial action is brought about. What is the *modus operandi* of skin-grafting? Does the transplanted morsel grow, multiplying itself by proliferation of its own cells? or does it merely act as a stimulus to the existing granulations, quickening them, and communicating to them the vital activity which they lacked before? That it grows by proliferation of its own rete mucosum is obvious from the case in which Mr. Bryant made grafts from a negro on to a white man, and the morsels of black skin grew and enlarged, retaining their dark colour. That the old granulations are in some way stimulated to new activity is obvious, from the fact that when grafts have been made on an old ulcer, points of healing spring up at the margins, and in other situations, at a distance from the engrafts. Thus we may take it for granted that the graft acts in both ways, and that its beneficial effect operates in two directions.

Perhaps a sufficient time has hardly yet elapsed since the introduction of skin-grafting to test the permanence of the



cures which are effected by its means. But all those who have had the largest experience of it are agreed that the resulting cicatrix is more soft and pliable—more like the natural skin—than an ordinary cicatrix. Upon this subject it is interesting to read Mr. Woodman's remarks :

“ My experience is (he says) that ulcers healed by this process (I do not mean where only one or two bits of skin have been put in as an experiment in a large ulcer, but where the grafts are within an inch of each other) remain well a considerably longer time than when cured in the ordinary manner. In burns also the contraction has been much less than usual, and I think a larger experience will confirm these views.”

And again he says—

“ Several cases of inveterate ulcers of the leg which were healed by grafting have returned for treatment after periods ranging from six months to two years ; in nearly if not quite all these cases the original spots at which grafts have been inserted have remained as small islands of skin, from the size of a pea to a sixpence, and in most of these, on merely giving rest and applying a simple zinc lotion, they have healed rapidly, the healing process taking place simultaneously from the edges of the wound and the islands of skin, these latter growing much as when first engrafted, but beginning to do so at once. This proves that, although the cicatrix resulting from engrafting is not equal to healthy skin, yet that a circle round each spot approaches so closely to true skin as to resist the ulceration to which the surrounding connective tissue succumbs.”

If it be so, this is a very great additional advantage. But even if it should prove that M. Reverdin's cicatrices are as liable to break down as any others, still it must be regarded as a great boon to have a simple means of stimulating and healing up those extensive and obstinate ulcers which have hitherto been such an opprobrium to surgery.

If it be a question whether we ought to take skin from one human being and transfer it to another, what shall we say of the practice of engrafting rabbits' skin on men? And yet this has been extensively carried out in Italy, as we learn from the pamphlet which is named at the head of this article. In some respects, perhaps, it is not so objectionable to have scraps of a rabbit's skin engrafted upon one's surface as it would be to have skin taken from a fellow creature, about whose freedom from infectious disease there might be a question. On the other hand, it would be very awkward if the fur—which, we are assured, perishes entirely—were to begin to grow ! Tufts of rabbit's fur on one's arm or leg would be worse than a tense and irritable cicatrix. The remedy would be more uncomfortable, more disturbing to one's mind, than the disease. We

cannot think, therefore, that the practice of taking grafts from rabbits—or indeed from any of the lower animals—will ever be tolerated in this country. Still we may give our readers a few examples of what is now being tried at Bologna, in Italy, extracted from Dr. Caselli's pamphlet :

“C. A—, æt. 58, was received into the hospital of St. Maria Nuova on account of a varicose ulcer on the internal surface of the lower third of the right leg. The ulcer was elliptical in form, 6 centimètres long by 8 wide, with edges more than 1 centimètre in thickness. As a last resource I performed transplantation on the 20th of August, following my accustomed method. I took four pieces of skin from his own thigh, and eight pieces from the belly of a rabbit, which had been previously shaven. I placed the little discs in three lines, putting those of human skin in the centre, and those of rabbit skin on either side. All the pieces adhered, and though those composed of human skin were more pliable they were somewhat raised above the level of the surrounding tissue, whereas the rabbit skin became incorporated with the epidermis, and led to the formation of very smooth, homogeneous cutis. Total cicatrization was obtained in twenty days, a result which, considering the size of the wound and the thickness of the edges, could not have been produced in four months by the usual method.”

P. L—, æt. 55, had been admitted into the hospital repeatedly for varicose ulcers, and subjected to various methods of treatment, but with no permanent benefit. Dr. Caselli, therefore, determined to try skin-grafting. He says :

“The patient had a great aversion to having the pieces of skin necessary for the operation taken from his own body. I therefore took eight pieces from the belly of a rabbit, raising them with depilatory forceps, and removing with spoon-shaped scissors the entire thickness of the skin. These I applied to the surface of the ulcer at the distance of 1 centimètre from the periphery, placing over them some discs of moistened cardboard, covering them with strips of waterproof, and dressing the wound afterwards as I have already described. In this case the result was not so brilliant as in the preceding, for at the first dressing I found that only one disc had adhered. But this went on gradually enlarging and uniting to the peripheral borders, which themselves, under the wet bandage, became narrower and drew closer so as to meet the graft. The cicatrization which resulted was excellent, and much more solid than in the preceding case.”

P. F—, æt. 60, entered the hospital on account of a very large abscess in the left leg, in October, 1871. He was treated with detergent injections and drainage tubes, and was discharged cured in May, 1872. He returned in the following November, to be treated for a varicose ulcer at the anterior and internal aspect of the same leg :



“The wound was 11 centimètres long, of irregular elliptical form, with its larger axis parallel with the limb. It measured 3 centimètres across in the upper part and 5 in the lower. The borders were hard, and raised more than a centimètre above the plane of the ulcer. For some days I applied strips of thin gutta percha to equalise the surface of the wound, and on the tenth day I undertook to transplant. I removed four pieces of skin by the process already described from the thigh of the patient, and applied them to the centre of the lower portion of the ulcer. I then took six pieces of the shaven skin of a rabbit’s belly, and placed all these on the upper part of the wound. I covered the whole with strips of cardboard, and completed the dressing as before described. In three days only the outer dressings were removed; on the fourth the whole was uncovered, and I saw with pleasure that all the discs were adherent and surrounded by an areola of proliferation. What has been the progress of these grafts in thirteen days you can see for yourselves—you find the patient almost cured. In the lower internal angle you will see a point which has resisted cicatrization, and which has a very high border. In this spot I purposely placed no graft in order to retain a recollection of the former condition of the entire ulcer, and so to be able to form an exact idea of the modifications produced in the borders of the wound when the cutaneous element is implanted at a short distance from them. You will be able, moreover, to form a comparison between the skin produced by rabbit and by human integument, and I believe you will agree with me in considering the former to be more smooth, more homogeneous, and of a more healthy hue than the latter. . . .

“I might relate six other cases which have had a like successful result in my private practice during the last few months, but, not to detain you longer, I will merely say that twenty-two pieces of skin were grafted on six individuals, resulting in the adhesion of thirteen, all of which were proliferous, and produced solid and regular cicatrization. The process adopted was always the same, and the epidermis became detached from the papillary structure in only seven discs.

“With one patient I tried transplanting the skin of a chicken as well as human skin; but the former did not adhere, while the latter made rapid progress.”

Of one case of human skin-grafting Dr. Caselli states—  
“I ought to observe that while in one of the two grafts the entire cutis adhered, in the other the epidermis became detached, and only the papillary surface remained adherent to the wound, as could be seen by the naked eye when looking obliquely at the surface.”

It will be seen from these extracts that Dr. Caselli is a very enterprising advocate of skin-grafting, and that he has tried grafts from both furred and feathered animals. It would be unjust to pass over his labours in silence, and yet we cannot say that they have added much to the value of M. Reverdin’s

practice. They have certainly shown that—apart from all moral objections—there is little or no advantage to be gained by taking skin from the lower animals. This negative value they possess, and we may accord him his due share of praise. On the other hand, his experience serves to enhance the value of M. Reverdin's method as originally set forth. Nothing is to be gained by resorting to rabbits or to chickens for our grafts, and, as we have already seen, the other modifications which have been proposed—such as scraping the epithelial scales of the skin, or taking large pieces half an inch in diameter—are alike futile. So that we must revert, after the experience of four years, to the method of skin-grafting first suggested as the best—viz., that in which small fragments, including some rete mucosum, are taken from the patient's own body, and implanted on the wound at intervals of about three quarters of an inch apart.

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#### XI.—Schiff on Inflammation.<sup>1</sup>

Dr. GUICHARD DE CHOISITY's translation embraces the following papers, most of which have appeared in the pages of the Italian journal, 'l'Imparziale.' Considerable condensation has been made.

1. "Lectures on Inflammation."
2. A memoir forwarded to the Academy of Florence, entitled, "Cellular Pathology and Inflammation; their Relation to each other."
3. A résumé of several lectures on "Venous Pressure as a Cause of Œdema."
4. "An Account of some Experiments respecting the Transfusion of Blood; made with the object of showing the Influence of the Nervous System on the Circulation."
5. "An Account of some further researches on the Cardiac Nerves."

Lastly, an important "Essay on the Vagus Nerve, considered as an Accelerator of the Cardiac Movements."

The abstract of M. Schiff's "Lectures on Inflammation" is disappointing, for instead of giving the Professor's views in regard to the nature of inflammation, it only gives his strictures on the views expressed by others. Up to a comparatively recent period, he says, those experimenters who studied pathological

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<sup>1</sup> *De l'Inflammation, et de la Circulation.* Par le Prof. M. SCHIFF, Traduction de l'Italien par le Dr. R. GUICHARD DE CHOISITY. Paris. 1873, pp. 96.



questions with the aid of the microscope, made use of the web of the frog, the peritoneum and ear of the rabbit, or the wing of the bat; and the phenomena they saw and so carefully described can still be verified. Any violent irritation first produces contraction of the vessel on which it acts, the contraction commencing at the point irritated, and propagating itself so to speak peristaltically in both directions, but peripherically as far as to the capillaries, which have no circular layer of fibres. The constriction occurring at the point irritated is followed by a dilatation which increases and becomes considerable. Most authors maintain that the circulation is accelerated during the first period, that of contraction, but others think the initial acceleration coincides with the commencement of the dilatation. The period of acceleration is followed by retardation of the blood current, the red globules no longer move in the axis of the vessels, but fill the whole of its cavity mixing themselves on the walls with the white corpuscles. The retardation becomes greater, the movement of the blood becomes slower, then oscillating, the excursions become fewer and fainter, and ultimately stagnation occurs; the vessels are then so gorged with red corpuscles that the outlines of the corpuscles cannot be distinguished. The smaller branches of the main vessels which usually only transmit liquor sanguinis contain red corpuscles, and the number of the vessels consequently appears to be augmented.

If the irritation have been moderate, all these phenomena gradually reappear in the inverse order till the circulation is eventually re-established.

Those who have written on inflammation, M. Schiff goes on to say, may be divided into three categories. One comprises those who, maintaining that vascular dilatation produces acceleration of the circulation (though it has in reality precisely the opposite effect), say that there is an augmentation between the blood and the inflamed tissue or parenchyma of the organ. They attribute to the parenchyma the same *initiation* in the inflammatory process that the author of the 'Cellular Pathology' assigns to the cellule. Some, proceeding from the notion that the force of the heart and the elasticity of the arteries are insufficient to maintain the circulation, have had recourse to a kind of polarity of organs attributing to the tissues a power of attraction for arterial and of repulsion for venous blood. Experiment, however, refutes this by showing that at the outset of an inflammation the current of the blood is accelerated in the arteries, though but slightly, and that it is simultaneously accelerated in the veins; the pressure is augmenting also simultaneously in both sets of vessels, the quantity of blood traversing

the part is greater by about a third. But if the above theory were true, there ought to be, under the influence of the initial augmentation of attraction, acceleration of the blood in the arteries and retardation of it in the veins; augmentation of pressure in the former, depression in the latter. By a second set of observers, amongst whom Henle may be mentioned, the *point de départ* of the inflammation is attributed to an alteration of the tissue of the blood-vessels permitting diffusion or exosmose to take place more easily, which again leads to their relaxation being deprived of part of their natural contents; a compression of them—a *passive* constriction of the walls is thus effected which may proceed to complete obliteration. This theory may be refuted by reference to the fact that in many inflammations the amount of fluid poured forth is not nearly sufficient to obliterate the vessels, and secondly, that the amount escaping through the walls can never be even approximatively equal to the quantity escaping from a divided vessel. Yet a vessel thus cut, especially if done with a sharp instrument contracts, the comparatively slight hæmorrhage being stopped essentially by the formation of a clot.

The third category, to which Schiff himself appears to belong, base their views on physiological facts, and especially on the absence of the middle or muscular coat in the capillaries, and its presence in the smaller arteries. The contraction of their coat is called into play by the nervous system, and, once induced, spreads in a peristaltic manner towards the capillaries. Supposing the constriction on arriving at the capillaries to intercept the blood current, all the phenomena described above would be accounted for, viz., first a slight acceleration at the constricted point followed by retardation, then accumulation of corpuscles, admixture of red with white corpuscles, distension of the vessel with augmentation of pressure, oscillations of the blood column synchronous with the beats of the heart, and finally arrest of the circulation; then, if relaxation of the vessel occur, the circulation re-commences, the vessel dilates beyond its normal size, and the course of the blood is more rapid.

The next chapter is on “Venous Pressure as a cause of Œdema.” In the normal condition, as is well known, the venous pressure is much inferior to the arterial, so that if, for example, the pressure in the crural artery and vein or other vessel of corresponding size be taken, the venous pressure will not be found to amount to more than a seventh, or even a ninth of the arterial pressure. If a ligature be applied to the principal vein of an extremity, the venous pressure is augmented, though to a less extent than might be expected, scarcely exceed-



ing a fourth or a fifth of the arterial pressure. If, however, the vein be the only channel by which the blood is returned to the heart, the pressure soon almost equals that of the artery. It is obvious, therefore, that the chief vein of a limb constitutes only one out of many channels by which the blood returns.

The question then arises, Is an obstruction to the venous current sufficient or not sufficient to produce an œdematous swelling of the part supplied by it? The answers given by different experimenters are various. Several declare that, after ligature of the crural vein in the inguinal canal, œdema of the lower limb ensues, and they adduce pathological evidence to the same effect. Others, on the other hand, declare that ligature of the crural or iliac vein rarely if ever causes œdema, and point to cases when even in man no œdema has resulted from strong pressure applied to the chief vein of a limb. Very lately M. Ranvier has published some interesting observations, in which he has shown that simple ligature of such a vein as the crural never produces an œdematous swelling, but if coincidentally severe lesion, causing paralysis of the nerves has been inflicted, enormous œdema may result. Simple section of the nerves has, however, no influence in causing œdema.

M. Ranvier maintains that in all cases where the obstacle to the flow of blood has caused an œdema there is at the same time a paralysis, or at least great depression of the nervous system, or of the vaso-motor nerves of the corresponding part. M. Schiff thinks that whilst Ranvier's explanation holds in many cases it does not do so in all, and that it is not sufficient for pathology; and he considers that his own experiments solve the difficulty. He dissects out the principal artery, and ties the whole extremity with the exception of the artery. All the veins are consequently tied. The pressure in the principal vein rises till it approximates that of the artery, and soon after œdema appears. Now, the œdema might be attributed to the obstacle to the circulation if two objections could be set aside. First, the ligature as applied above includes the nerves; and secondly, it arrests the circulation through the lymphatics. Certainly the nerves are not so much compressed as entirely to arrest the conduction of nervous influence, but it would to some extent at least enfeeble them. Schiff, therefore, tied the limb, leaving the arteries and nerves free. The power of movement and sensibility were retained for some time, but after some hours in warm-blooded animals and after a day and a half in frogs œdema appeared. Again he tied the lymphatics of the posterior limb (in frogs) alone, but no œdema occurred, even when the crural vein was also tied. He hence draws the conclusion that the production of œdema after

ligature of the limb is independent of the ligature of the nerves and lymphatics. In another series of experiments made upon dogs he tied the crural vein and a certain but variable number of the collateral veins, and arrived at the important conclusion that when the veins tied were sufficiently numerous to cause the pressure in the principal vein to rise to six tenths of the arterial pressure, œdema appeared. He was ultimately enabled by attention to this point to predict whether œdema would or would not follow the operation. The different results mentioned above, as having been obtained by different experimenters, is explicable on the supposition that some have tied the vessel rather higher than others, so as to arrest the circulation in a larger number of collateral veins. In regard to Ranvier's views there can be no doubt that after paralysis of the vaso-motor nerves the small vessels dilate. The peripheric circulation experiences less resistance, and the pressure in the veins becomes higher than natural. If to this is superadded the increase of pressure consequent on ligature of the principal vein, the pressure in the veins may rise to two thirds of the arterial pressure, and then œdema is sure to follow.

The next paper deals with the subject of the "Influence of the Nervous System on the Circulation." Prof. Schiff, on commencing a new series of researches on the innervation of the heart, desired to determine, in the first place, what is the cause of the apparent weakening of the heart which manifests itself after the destruction of the spinal cord, or after division of it just below the medulla oblongata. This weakening consists, 1st, in diminution of the pressure of the blood, which commences after a short period of excitation consecutive upon lesions of nerve centres, and continues to be more distinctly as well as more quickly marked till it reaches a minimum; and 2nd, in a diminution of the frequency of the pulse, which becomes slower in proportion as the pressure falls up to a certain point, when it ceases to fall though the pressure continues to diminish. Von Bezold attributed the diminution of pressure to direct weakening of the energy of the heart, but Goltz, Ludwig, and Thiry have with much probability attributed the fall of the pressure to vascular paralysis and dilatation of the small vessels consequent on the fact that all the vaso-motor nerves of the body arise, as Schiff showed in 1855, in the medulla oblongata, and run in the central portion of the spinal cord. Ludwig and Thiry in the explanation they gave of the fact only take into account the paralysis of the small vessels between the large arteries and veins, and the contraction of which is the cause of the pressure in the arterial system. It is evident that when these small vessels are dilated in consequence



of the paralysis the arterial current finds a freer passage into the veins. The pressure of the arterial blood ought then to diminish even if the force of the heart remained unaltered, and the diminution of the blood pressure causes, according to these authors, a retardation of the frequency of the pulse. Whilst admitting this, Schiff calls attention to the fact that the large vessels themselves in many, and perhaps all, parts of the body become relaxed, and dilate after paralysis of the vascular nerves. Hence, he agrees with Goltz in believing that the principal cause of the apparent debility of the heart, and frequent diminution of pressure, does not lie essentially in the facility with which the arterial blood traverses the dilated vessels of the venous system, but that it is chiefly owing to the fact that the large vessels, which are themselves dilated, are capable of retaining a large quantity of blood, which being incapable of returning to the heart is practically subtracted from the circulation. The animal in this situation is consequently anæmic, and if this be the correct explanation of the phenomena we ought to be able to restore the power of the heart by injecting a quantity of blood of another animal of the same species sufficient to compensate for the increased capacity of the vascular system. Very numerous experiments were made to test the correctness of this view, and the arrangements required being very complicated (artificial respiration having to be maintained, the movements of a manometer observed, &c.), various modifications had to be introduced. Schiff was greatly surprised at the immense quantity of blood that could be injected into an animal with paralysed vascular system without restoring the tension existing prior to the paralysis, and he fully expected to find extravasations of blood, none, however, were found at the post-mortem examinations. In the later experiments after the section of the spinal cord had been made and all arrangements completed, the vascular system of a second and strong animal was connected with that of the animal operated on, and blood was allowed to flow into it in small quantities at a time till a very large amount had been introduced. It was then found that as each fresh quantity of blood was introduced the pressure in the vessels of the animal increased more and more till it attained the original point that it had before the section, and remained at this point for a time sufficiently long to show that the debility of the heart's action after the section of the cord was in reality due to the increased capacity of the vascular system and a deficiency in the relative supply of the blood. That the augmentation of pressure was not due to the fluid injected but essentially to the action of the heart was shown conclusively by the circumstance that when the pressure after

injection of blood had regained its normal amount, if the vagi were eliminated by an interrupted electrical stimulus the heart temporarily ceased to contract, and the pressure of the blood, registered by the manometer, was observed to fall immediately and rapidly towards its minimum. In a dog weighing about eleven pounds the amount of blood which had to be injected after section of the cord to restore the pressure to its original amount in the course of half an hour or forty minutes was from 230 to 300 grammes. The greater part of the blood was found to be collected in the veins. It is evident, therefore, that under ordinary circumstances the veins are in a state of considerable contraction. A *résumé* of the paper on the "Cardiac Nerves" will be found in the Chronicle of Physiology published in this number of our Journal.

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## XII.—Conservation of Energy.<sup>1</sup>

"Knowest thou any corner of the world where, at least, FORCE is not?" . . . .  
 "Thinkest thou that there is aught motionless—without FORCE, and utterly dead?"—SARTOR RESARTUS.

THE assertion that "all the errors of man are physical errors" cannot be too often repeated. "Our imperfect ideas of matter, of its properties, of its combinations, of its power, of its mode of action, or the energies which spring from it are due to a want of experience." From the earliest times even to the present day there has been, and there is, a wide-spread antipathy to natural research. There is little difference indeed between the masses now and man in early times so beautifully described by Ruskin and with his characteristic genius: "Taking very little interest in anything but what belonged to humanity; caring in no wise for the external world, except as it influenced his own destiny; honouring the lightning because it could strike him, the sea because it could drown him, the fountains because they gave him drink, and the grass because it yielded him seed; but utterly incapable of feeling any special happiness in the love of such things, or any earnest emotion about them, considered as separate from man, &c. The man of science alone, and not the masses, fulfil the opposite condition. Now he has deep interest in the abstract natures of things, inquires as eagerly into the laws which regulate the economy of the material world as into those of his own being, and manifests as passionate admiration of inanimate objects, closely resembling, in its elevation and

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<sup>1</sup> *The Conservation of Energy, an Elementary Treatise on Energy and its Laws.* By BALFOUR STEWART, M.A., LL.D., F.R.S., Professor of Natural Philosophy at the Owen's College, Manchester.



tenderness, the affection which he bears to those living souls with which he is brought into the nearest fellowship." Here indeed we have the two sides of the picture—the one indicating an ignorance of, not more profound than the indifference manifested towards, nature's works and wonders; the other exhibiting unbounded love and admiration of everything within the comprehension of the senses, but not contented like one who gazes on a beautiful painting, careless and indifferent as to who painted it, but as one eager to know the "how," "the why," and "the wherefore." Humanity from the earliest times to the present day has been much in the same condition as a lunatic—permanently bereft of reason with occasional lucid intervals. The brightest spot, regarded from a scientific point of view, is that which dawned over the once powerful states of Greece, which commenced with the great father of medicine, Hippocrates, whose mantle fell upon a successive race of scientific heroes—heroes greater than the fabled warriors depicted by the greatest poet of all times, Homer—heroes indeed because they battled not with the muscularity of men of like passions with themselves, but with the giant laws of nature, endeavouring, in a poor and feeble manner, but with an earnestness worthy of the cause, to elucidate the problems which perplexed them. It must be granted that the ancient philosophers knew very little, and nothing absolutely. They depended too much upon speculation, and sadly too little upon observation and experiment. However, they speculated in the right direction, and the theories which they advanced are still living in the minds of far greater men, and in some measure act as beacon lights to observers of the present day. The philosophers admitted two primary principles—one a passive, or matter; the other an active power which in some unknown manner influence the passive. They believed that the latter was possessed of universal energy, and the former (matter) was susceptible of all forms and qualities. It will thus be inferred that matter was regarded as passive or inert, and that activity was produced in the inert by the influence of some external power. Unfortunately, this power was believed to be derived from either supernatural beings or some metaphysical entities. Aristotle, in his 'Physics,' says:—"Everything that is moved must of necessity be moved by something else; and that thing is moved by something that is moved either by another thing, or not by another thing. If it be moved by that which is moved by another, we must of necessity come to some Prime Mover, that is not moved by another. For it is impossible that what moveth, and is moved by another, should proceed *in infinitum*." This is a fair specimen of the reasoning adopted by

speculative men. It approaches more to the metaphysical character of legal language than that of the plain, honest descriptions which scientific men give of facts now. It was, nevertheless, a great point gained to the history of science that a foundation should have been laid upon so simple a basis as matter and energy. But for centuries afterwards not a single additional fact was contributed to it. The builders had left it to perish, either from liability to continue the work, or from that which is worse than inability—fear.

The next chief step in the history of Force may be embodied in the three great laws discovered respectively by Kepler, Galileo, and Newton. Kepler found that bodies move in a right line, and with uniform velocity when impelled by a single force. Newton—that when one body is moved by another, the reaction is such that the second loses precisely as much motion, in proportion to its masses, as the first gains—in other words, there is a constant equality of action and reaction. Galileo's law stands much in the same relationship to the theory of Copernicus as Darwinism does to the special creation doctrine. It is the law of "coexistence of forces"—viz., that any motion common to all the bodies of any system whatever does not affect the particular motions of these bodies with regard to each other, which motions proceed as if the system were motionless."

It is almost impossible, as Comte says, to conceive of any case which is not met by these three laws of Kepler, of Newton, and of Galileo. The facts which at a later period resulted from a broad comprehension of the above-mentioned laws may be briefly stated. 1. There are two kinds of motion—a visible, or the motion of a mass from one to another locus, and an invisible or concealed motion, the latter depending upon the energy resident in the component particles or molecules of a body. 2. These motions were known as *acquired* and *spontaneous*. The latter was abandoned as untenable. 3. Motion, again, was divided into simple and compound: the former being excited in a body by a single cause, and the latter by two or more equal or unequal causes. 4. As the knowledge of motion increased, men began boldly to declare that everything in the universe is in motion; that the essence of matter is to act; and that there is not a single molecule in an absolute state of rest. 5. Because a body seemed at rest it was held to be merely a deception of the senses; and it was maintained that there was a concealed force called the *vis inertia*—in fact, a *nisus* by which the individual molecules of a mass press one upon another, reciprocally resisting each other, acting and reacting incessantly, and that these actions, reactions, &c., maintained the molecules in a state of apposition, thereby forming



a mass which, viewed as a whole, had the semblance of rest—a condition regarded simply as the equality of the motion—actually no one of its particles having ceased to be in motion for a single instant. 6. Without this continual motion nothing could either be produced or preserved.

There is one striking and remarkable fact apparent in analysing that ancient and comparatively modern philosophers really did know, and that is, while all admit an active power or energy, not one of them had any actual conception what that factor or power was, or from what source it was derived. It may be said that the Greek philosophers taught that compressed fire produced air, &c., but it must be admitted that the Greek philosophers taught a good deal of nonsense. We might as well say with the French philosopher, “Elementary fire appears to be in nature the principle of activity; it may be compared to a fruitful leaven, that puts the mass into fermentation and gives life”—knew the sources of force as chemists and physicists know them now. While admitting that these men sailed closely by the harbour of fact, it is impossible for us to assume that they actually anchored within the harbour. If we fathom what they really meant by what they have said, we should find that what they knew was at the best fireside hypothesis. They were just as far from the actual truth as the prophet Moses from the promised land when on the summit of Mount Nebo. Carlyle has embraced the whole subject more comprehensively than all the ancient philosophers put together. Teufelsdröckh riding through the Schwarznold, his horse having cast a shoe, espies the bright fire of a smith’s shop across the moor and thus contemplates it: “That smithy fire was first kindled at the sun; is fed by air that circulates from before Noah’s Deluge, from beyond the Dogstar; therein, with Iron Force, and Coal Force, and the far stranger Force of man, are cunning affinities and battles and victories of Force brought about: it is a little ganglion, or nervous system, in the great vital system of immensity.” “The withered leaf is not dead and lost, there are Forces in it and around it, though working in inverse order: else how could it rot?”

But Carlyle is not a scientific man though he has grasped the great doctrine or law of Force. The next true phase in the history of Conservation of Energy is that which has been so ably and lucidly described by Helmholtz. The researches of Rumford, Davy, and Montgolfier had added materially towards a more general conception of the great law. But the first to compass the clear and distinct idea of this law, and to venture to pronounce its absolute universality, was Mayer, of Heilbronn, and simultaneously and independently of him Joule, of Manchester,

of whom, as Professor Roscoe says, he is better known on the Continent than in his own city. For Galileo's law of the intensity of force a new mechanical idea was elaborated—the "Quantity of Force," or the "Quantity of Work" or of "Energy."

In the book under review the author has exhaustively dealt with this great and important law. He has successively described—and improved the description by simple illustrations—from the starting-point of what "energy" is, through its various phases of conservation, transmutation or translation and dissipation of energy. The book is evidently written, not to attract the scientific world so much as the illiterate. In other words, it is written in a popular style, and all the examples given to illustrate "energy" are drawn from familiar sources, and described in as intelligible language as it is possible to produce, in order that the book with its valuable freight may find its way into the homes and hearts of the masses. But although it is popularly written, it is a valuable book to the scientific man. Howsoever familiar one may be with the subject, information can always be derived by a perusal of the labours of a master of the science of which he treats. Dr. Balfour Stewart needs little praise from us. His status as a physical philosopher is such that nothing that we can say can elevate him to a higher pinnacle in the noble path which he has chosen.

If we presumed to criticise at all, the criticism would fall upon certain peculiar ideas which the professor introduces in his first chapter. But as these have been so lightly touched upon, and evidently not been regarded as essential points of importance, we may fairly let them pass for what they are worth. There can be no doubt of this, the book is a good one; much has been written on the subject, but no other book, familiar to us, has embraced the subject of "energy" in so exhaustive and comprehensive a manner. It is rarely that we read a book with as much interest and pleasure as we have read this one of Balfour Stewart's.

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### XIII.—Modern French Surgery.

(*Concluded from p. 378.*)

IN reviewing the present position of surgery in France there are so many advances to be noted both in science and practice which we owe directly to French genius that it is not easy to single out representative specimens which, in showing us modern French practice, shall at the same time remind us of



what share the surgeons of that country have contributed to the developments of which we are all reaping the benefit. We shall therefore follow the principle adopted in the earlier portion of this article, and content ourselves with some reference to two branches of practice which have of late years been forced into comparative prominence—namely, Cancer and Stone in the Bladder.

The pathology and treatment of tumours will probably long continue to be a fertile field for further active exploration; for in spite of the number of enthusiastic workers, as well in Germany and in our own land as in France, the increase of knowledge available for the practical surgeon is still very small. At the late important debate on cancer in the Pathological Society of London it was almost bewildering to follow the various expressions of opinion upon the pathology of the disease, and to remember at the same time that all these different views were founded upon the same recorded observations. The subject is so important that it may be worth while to try to state plainly in a few words the present position of the controversy amongst pathologists and its practical bearing upon treatment, and we shall be then enabled at the same time to see in what respects surgery is indebted to France for light thrown upon this department of study.

A widespread notion prevails, and we see is shared by M. Gosselin, that histologists have done little or nothing for practical surgery, because any classification of tumours suggested by them, however satisfactory from a scientific point of view, is yet unavailable for the surgeon in his clinical work. This is clearly put by M. Gosselin,<sup>1</sup> who, speaking of the difficulties of reconciling in one classification both the clinical and the anatomical characteristics of tumours, points out that at one time it was hoped that these difficulties might be surmounted when histologists began to recognise such minute structural peculiarities as should confirm either the one or the other.

The first efforts of Lebert tended in this direction, and all seemed hopeful enough when as a result of these investigations it appeared that malignant tumours contained elements having no analogy in the normal organization, whilst innocent tumours were made up of elements corresponding to those met with in other parts of the body. If we could have rested here surgery might have been well content with so powerful an auxiliary as the microscope. But later researches, and notably those of Müller and Virchow, showed that there are no tumours whose elements are wholly different from those encountered elsewhere

<sup>1</sup> 'Clinique Chirurgicale de l'Hôpital de la Charité,' par L. Gosselin, Paris, 1873.

in the body, but only that in some growths, and specially in cancers, the cellular elements are comparable rather to those of the embryo than of fully-developed organs. Even this would have furnished some sort of standing ground had not still more recent investigations, of which the most important in France have been those of MM. Cornil and Ranvier,<sup>1</sup> made it plain both that many malignant tumours have no such embryonic elements and that certain benign tumours do display them in greater or less abundance.

Further, if recent microscopic research had provided us with a definite classification of tumours available to the surgeon in doubtful cases immediately after removal of the growth, *that* would be a considerable boon. But as a matter of fact there are many tumours—and these amongst the most doubtful—upon which the modern histologist will pronounce no verdict until some days have been consumed in hardening a morsel of the growth in appropriate reagents, since it is now clear that the precise form of the component cellular elements of a given tumour is often of far less consequence than the arrangement of these in the mass and their mutual relations.

It is natural, of course, for the practical surgeon to look for some clinical interpretation of the work when the pathologist has succeeded in forming a sound scientific classification of tumours, but it must be confessed that histologists hitherto have not been able to satisfy these natural requirements.

They have indeed shown that tumours having a given minute structure are far more likely to return and to be attended with other like growths in other parts of the body than are those having another and a simpler structure, but when one comes to sift this knowledge it is found that surgeons were able to predict nearly as much before merely by comparison of certain rough physical characters. Thus, the histologist tells us that a fatty tumour is an innocent growth, and as the surgeon long ago found that out for himself the information is not very valuable. Histology teaches us also that a scirrhus tumour is exceedingly malignant, but it is only in those rare instances in which the scirrhus nature cannot be detected without microscopic aid that this information is important, since the fact had long been known. And in the doubtful cases, such as certain of the sarcomata for instance, microscopic examination leaves the clinical future still in as much doubt as when the same growths were spoken of as fibro-plastic or encephaloid tumours.

Nevertheless, the patient labours of such pathologists as

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<sup>1</sup> 'Manuel d'Histologie Pathologique.' Paris, 1869.



MM. Cornil and Ranvier (for we speak now almost only of French work) would not be without point if they only afforded the sole reliable data on which to found a scientific classification of new growths. But we believe that histology has done infinitely more than this, and it is because this higher work is still hardly recognised even by the workers themselves that we wish to direct attention to it.

To understand what is here meant it is only needful to glance at Sir James Paget's eloquent and powerful defence of his views upon cancer at the debate already alluded to. During the whole of that speech recent histological work was kept in the background, and the result of this was to render the old mysteries surrounding the multiplication of cancer in the body so inexplicable that nothing short of a general change in the blood or some other all-pervading tissue could account for them, and surgeons were exhorted to turn their attention from all local remedies—as free excision of the tumours—as useless, and to begin again to grope about for some drug which should have an anti-cancerous action and combat this supposed general taint just as mercury is thought to combat the poison of syphilis. Can any suggestion be more discouraging to modern surgeons, or more likely to play into the hands of empirics, who derive nearly all their gains from an unscrupulous employment of such arguments?

Now, a careful perusal of such a work as that of MM. Cornil and Ranvier (and there are now a great many books of the kind both in this country and abroad) will make evident certain points about cancer which go far to clear away the mystery hitherto attending its manifestations, if they do not afford material aid in their subjection. Thus, it seems to be generally admitted by observers that there is always some one growth in advance of all the rest in a cancerous outbreak, and that the minute anatomical conformation of the secondary growths, wherever situate, corresponds closely to that of the first or primary tumour. It has been further shown that the secondary tumours for the most part affect special regions and these in a special order—as the implication of the lymphatic glands in regular succession, in a case of scirrhus breast before the appearance of visceral complications—an order, namely, which points pretty clearly to a mechanical transference of something—either fluid or corpuscular—along certain given channels from the primary seat of disease. Without going further into the matter at present, it is evident that these facts alone go far to explain the more extensive multiplication of some kinds of tumours than others, and if it be further admitted that this multiplication bears a direct relation to the mechanical facilities

for the conveyance of these germs from the first tumour to remote parts—a statement which has been publicly made although not yet generally confirmed by histologists—then it would seem fair to assume that the only real distinction between cancerous or malignant and benign tumours is a distinction depending upon the power of distribution throughout the system of germinative particles from the primary seat of mischief. Hereditary taint does not enter into the question, as all tumours seem to be about equally hereditary, and the *mode* of inheritance, a point strongly insisted on by Sir James Paget, may be capable of a histological explanation. Nor does “cachexia” affect the argument materially, since of late years the ablest surgeons have been least disposed to recognise “cachexia” as being in any sense more suggestive of cancer than of any other wasting disease.

It will be evident that in thus claiming for histology that it has thrown light upon the nature of cancer and its affinities with other tumours, it is not suggested that it has rendered cancer a whit more tractable; but it will be probably admitted that it has at least shown us that we must be guided in our treatment of this terrible disease by the same principles which dictate our treatment of the simplest tumour. Its greater energy of growth and fatal facility for distribution should only make us the more anxious for its early uprooting or alleviation; and there is no more reason for our prosecuting a search after some general remedy, as mercury or iodine, for cancer, than for any fatty or cartilaginous outgrowth, save the reason furnished by the greater gravity of the former disease owing to its unfortunate tendency to scatter its seeds.

Before referring to the modern French method of using caustics in the removal of tumours, allusion may be made to a pretty application of physics to diagnosis by M. Hénocque.<sup>1</sup> In most cases of doubtfully malignant tumours the diagnosis lies between syphilitic gumma, lipoma, and sarcoma. The treatment is different in each case, iodine with or without mercury rarely failing to disperse a gummy nodule, whilst the other two are only, so far as we at present know, to be met by local measures. But the operation for sarcoma ought to be very different from that for lipoma. Lipoma only needs cutting down upon and “shelling out,” but when this is attempted (as it often is) with sarcoma, the result is a speedy local return of the disease. Under these circumstances M. Hénocque’s suggestion is valuable. It is to freeze the tumour about which there is doubt. The fatty tumour will at once become quite

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<sup>1</sup> ‘Sur les lipômes et la diathèse lipomateuse,’ par Dr. Darbez. Paris, 1868.



hard, whilst the consistence of other growths will be hardly affected.

Reference has been already made, in the earlier part of this article, to the frequency and fatality of erysipelas and its allied diseases after operations performed in Paris. It is mainly on this account that the Paris surgeons have practised the removal of cancerous tumours by caustics rather than by the knife so far more generally than we have done in England. M. Gosselin has published some interesting statistics on this point. It appears that between the years 1862 and 1873, that surgeon amputated the breast for cancer thirty-six times (the operation being followed by erysipelas in twenty-one instances), twelve times with a fatal result. It may be remarked, in passing, with reference to what has been already said on the subject, that the greater proportion of these accidents happened in private practice. Of twenty-two hospital patients eleven took erysipelas and six died; whilst of only fourteen private patients as many as ten took erysipelas and six of these also died, four of the number having been operated upon in the country and under the best hygienic conditions.

On the other hand, M. Gosselin has in the same time removed cancerous breasts by caustics twenty-five times, sixteen in the hospital and nine in private, of which number only two took erysipelas (both hospital cases) and one died. Of thirty-six operation cases, therefore, twelve died, whilst only one death followed the application of caustics in twenty-five cases. Under these circumstances it is not surprising that the French surgeons, for the most part, prefer the slower and infinitely more painful process of destroying cancers by caustics, to the comparatively speedy and less painful use of the bistoury, reserving this last measure for very large tumours in patients who are in good health otherwise, and who are anxious to avoid the prolonged suffering caused by the caustic. The practice of thoroughly benumbing the part by prolonged congelation before applying the caustics, although its safety and efficacy has been long since demonstrated by one of our own countrymen, has not apparently been employed in France.

The mode of procedure introduced by M. Maissonneuve, and commonly practised in France, differs, as is well known, from that with which English surgeons are familiar, in the employment of small darts, composed of one part of chloride of zinc to two of flour and water, hardened by prolonged exposure to a gentle heat. These darts are plunged into the tumour at certain distances and to a certain depth, the bistoury dividing the superficial structures to permit of their entry, and the direction and number of the darts being regulated by the size of

the mass to be removed. The extent of the destructive action of each dart being known, the only care of the surgeon is to put them close enough to leave no diseased tissue free from their influence, and at the same time to prevent any serious accident that might be caused from the caustic penetrating too deeply. M. Bauchet reported a case before the Société de Chirurgie some years ago in which the pleural cavity had been opened in this way, and it is well known that the like accident happened more than once, in the practice of English surgeons, soon after the renewed zeal in the treatment of cancer by caustics was displayed in consequence of Dr. Fell's experiments at the Middlesex Hospital some sixteen years back.

If now we glance, in conclusion, at the surgery of vesical calculus, the history of the gradual development of this important branch of practice strikes us at once with the number of French names associated with the several improvements. If we have reason to be proud of our own country in making such a retrospect—and, indeed, Cheselden has been followed by a not unworthy list of distinguished disciples—surely the French have abundant reason to congratulate themselves. But it would be wearisome to recapitulate here the various steps in advance which French surgeons have made in the treatment of stone in the bladder. We will rather refer to two or three points which are perhaps less commonly mentioned by English writers, but which are not unsuitable for a place in such a sketch as the present.

Lithotrity seems to be just now very generally preferred to any cutting operation, and without doubt it is an excellent mode of treatment in appropriate cases. But no one can have observed many cases of lithotrity without being struck by the wide difference in the results of the same operation on different patients. In the one set of cases the patient suffers at most a little pain during the crushing, and a little more in the subsequent passage of fragments and débris, with perhaps some slight febrile disturbance with rigors. But in other cases severe rigors, following swiftly upon the operation, usher in intense fever, with dry tongue, muttering delirium, swollen belly, and speedy death. These formidable symptoms seem to be mainly due to the laceration of the mucous membrane of the urinary passages by instruments or fragments of the broken stone, and it is therefore, that the rule followed by Gosselin and the best French surgeons is to select only those cases for lithotrity in which the mucous membrane is not greatly congested and thickened, and so liable to tear and bleed at the least contact with instruments, in which, moreover, the urethra being freely patent both instruments and fragments may have



ready exit, and in which, also, the bladder is capable of such distension as not to interfere with the free movements of the necessary instruments, and risk being caught and torn by the lithotrite. If such a selection of cases be judiciously made, and if due care be exercised in the delicate manipulations required, the particular form of instruments does not appear to be an important element of success.

The first ingenious suggestion of Heurteloup which rendered lithotrity so easy has been modified and improved by many other operators and ingenious instrument makers. For, besides Civiale himself, who takes so prominent a place in the history of the operation, Guillon, Charrière, and Robert and Colin, have all made important suggestions, perhaps the most convenient form of all being that proposed by our own distinguished lithotritist, Sir Henry Thompson. But all these variations in the mode of crushing the stone are commonly quoted in our text-books, and need not be enlarged on here.

The *perineal lithotrity* of M. Dolbeau, in which lithotrity is combined with median section of the urethra and dilatation of the neck of the bladder and prostate, was at first attended with such remarkable success in the hands of that eminent surgeon as to attract a considerable amount of attention abroad, although little noticed in this country. After a striking series of successful cases, without a single death, M. Dolbeau met with some less encouraging results—perhaps from his not clearly intelligible practice of combining the crushing with the dilatation in every case—and at present his operation is not meeting with very general support. The suspicion entertained by some leading authorities that the dilatation too often means a tearing of the neck of the bladder also stands in the way of M. Dolbeau's plan, for it is contended that such tearing is as hurtful as cutting. In one case of death from sloughing erysipelas soon after the operation it was found that the dilator had really torn the neck of the bladder, and had so probably directly contributed to the fatal result. It would be premature, however, to form any judgment upon this proceeding until we have more cases upon which to found an opinion.

In the cutting operations for removal of stone perhaps the most noteworthy improvements have been suggested by M. Vidal de Cassis and by Dupuytren.

M. Vidal, desiring to lessen the chance of urinary infiltration in the supra-pubic operation, proposes to defer cutting into the bladder until, by the action of Vienna paste, adhesions have been brought about between the bladder and the abdominal wall. But the practical difficulties which occur to one at once, in securing by such means a fixation of the movable bladder to the abdo-

minal wall, need an extended experience to convince surgeons of their capability of being overcome, and this extended experience is not forthcoming, although M. Vidal has occasionally practised the method.

The double lithotome of Dupuytren, on the other hand—clearly suggested by the single-bladed *lithôtome caché* of Frère Côme—is one of the most ingenious instruments of modern surgery, and is much prized by French surgeons for the removal of very large stones, any bleeding being arrested by the *canule à chemise* of the same distinguished surgeon. And when due care is taken, as by M. Nélaton, to pass close in front of the rectum without wounding it, the bulb of the urethra may escape lesion altogether, and so the risk of extensive suppuration of spongy and venous tissue be avoided. Practical surgeons know how frequently secondary hæmorrhage attends all cutting operations for stone, and the *canule à chemise* is widely valued and extensively used in consequence.

It seems hardly right to close this glance at the work done by French surgeons in our day without some reference to their labours in the advancement of our knowledge of the pathology and treatment of syphilis, but, indeed, we owe to them so much in connection with this branch of our subject, and the disease itself is so important and complicated in its manifestations, that it is hopeless to attempt to touch upon it in the course of any single article, and we must leave it, as we have had to leave so many other interesting matters, not because we fail to appreciate the valuable services rendered to science by our Continental brethren, but because we have not the space to do them the justice those services demand.



## Bibliographical Record.

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**Paterson's Life of Syme.**<sup>1</sup>—The subject of this memoir has left a lasting name in the science and practice of surgery. But apart from this his history has been so mixed up with that of many men of mark, and with many public incidents in which he took an active part, that a narrative of his life was to be looked for, and was, moreover, desirable in order that a correct conception of his character and conduct might be formed, particularly by many of his contemporaries, whose knowledge of him was pretty much confined to that of his peculiarities, which, on the surface at least, appeared to be not the most amiable. Indeed, his name has been so largely associated with scientific controversies and with personal conflicts, that justice to him as a surgeon of admittedly the highest talents imperatively called for a portraiture of the man, and a history of the circumstances in which he was placed and of the motives and principles by which he was governed.

The task of doing this has been undertaken by Dr. Paterson, who has evidently sought to perform it honestly, and has produced a volume that will be read both with interest and pleasure; nevertheless, it will be generally felt that he has not succeeded in presenting a complete and satisfactory portrait of the eminent surgeon. He has too prominently kept in view the controversial aspect of the subject of his memorials, and has missed to fairly represent those characteristics which rendered him an object of lasting esteem and sincere regard by a large body of friends.

Yet Dr. Paterson has pursued the right course of using Syme's letters and other documents, together with letters that passed between him and various friends and others, to endeavour to correctly appreciate his character; and although a great admirer of the surgical talents of Syme and of his private worth, he is nevertheless no indiscreet flatterer or injudicious apologist.

From what we have said, it may be gathered that an account of Syme's controversies constitutes a special feature of these

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<sup>1</sup> *Memorials of the Life of James Syme, Professor of Clinical Surgery in the University of Edinburgh, &c.* By ROBERT PATERSON, M.D., &c. Edinburgh, 1874.

“memorials,” and in regard to it we may observe that the details given contribute to place many events in a clearer light than has hitherto been attained ; although it does not remove altogether the conviction that the great surgeon was often intemperate in writing and speech, pugnacious and overbearing towards others, and exceedingly irritable in matters affecting himself and his doings, looking with ill grace on any seeming intrusion upon his assumed rights, or any questioning of the correctness of his own opinions. In short, as Dr. Paterson says, “he was a man of war in the fullest sense of the term, doing battle for principles, and without any aim at self-aggrandisement or advantage.” But to this it may be added that he was a man of war not always judicious in the fight, or fair to his antagonists. Born in Edinburgh, he was the son of a man described as of great acuteness and sagacity, allied with obstinacy and perseverance. These qualities of character the son eminently inherited, and added to them largely of the passion of jealousy.

In the matter of the disagreement between Liston and Syme, there was, in Dr. Paterson’s opinion, no open quarrel, but rather an “increase in dislike, which had its origin in mutual jealousy.” This account of the origin may be correct, but few readers, we apprehend, will so qualify the term to be employed as not to designate the affair an open quarrel, particularly at its culminating point, when Syme commenced an action at law against Liston on account of the insulting entry the latter made in the subscription book of Syme’s ‘Minto House Surgical Hospital.’ For this unjustifiable act Liston made ample apology and the action was stayed, yet with him it was at least a decided quarrel and manifested as such. His impulsive and demonstrative character made him break loose from restraints which held in check his more wary, reticent, and less openly perturbable rival, and threw over his jealousies and quarrels a shadow of patience and quiet endurance.

Dr. Paterson enters on a comparison between these two great surgeons, and would discover between them many external resemblances. But to our mind the resemblances so called are rather examples of coincidences in circumstances between two men engaged in the same pursuits and ambitious of the same objects at the same time. Had we a life of Liston before us we believe we could much more effectually prove a great divergency in character between the two. Indeed, the differences of their tastes for amusements referred to by Dr. Paterson—the one loving field sports, the other the quiet pursuit of botany, when at leisure—alone speak more plainly for dissimilarity between the two men than do all the incidental circumstances brought together by the biographer for resemblances.

The determination of character and the self-reliance of Syme are illustrated by many events in his chequered and stormy life. To cite two or three instances by way of example, we may first



point to the bold act when, upon his misunderstanding with Liston, and when still a very young man only recently embarked in practice and of slender resources, he started a new medical school in opposition to his late colleague, then at the height of his fame as a lecturer. The same resolute character led him to annul his connection with his newly formed school, because he considered his colleagues would fetter his liberty of action; but a still more daring act was the institution of the Minto House Surgical Hospital on his failure to secure a post in the Royal Infirmary. The like traits of character were displayed in his taking up surgery as his special subject, contrary to the advice of his friends and regardless of their fears for his success, confronted, as he was, by distinguished and popular rivals. And to quote one more illustration of those qualities, we may mention the removal to London and the speedy resignation of his position and prospects there and his return to Edinburgh.

This last-named episode has presented to most minds a highly romantic character, and various ideas have been entertained regarding it, one particularly prevailing, viz., that Syme was disappointed with his prospects in London practice. To this notion Dr. Paterson gives a denial, and in the letters that passed between Syme and some friends, now published, we may discover the true interpretation of the transaction.

His return to Edinburgh seemed generally welcomed, and he was speedily reinstated in the appointments he had so recently resigned at the university and the infirmary. He devoted himself with fresh ardour to his profession and maintained his high reputation as a clinical teacher, a position in which he had no superior. Honorary distinctions flowed in upon him in recognition of his eminent talents as a surgeon; and, apart from the broils with colleagues and others into which he was perpetually precipitated, his career was one of great success, whilst his private friendship was sought and valued by many of worth and position both in the profession and outside it. He did not carry the annoyances of professional life into the family circle, but there exhibited himself as an affectionate husband and father, a warm, genial, and hospitable host.

It would not be right to dismiss this volume from further notice without stating that, besides controversial and purely personal details, it presents a sketch of the principles of practice adopted by Syme, and of the many advances made by him in operative surgery; his own descriptions and histories being largely used, so that, apart from its value as a biography, it possesses also that of a history of surgery during the period embraced in the life commemorated.

**Wilson's Guide to Zoology.**<sup>1</sup>—Mr. Wilson's work, although an avowed compilation, marks a certain era in zoological science. It is devoted to an epitome of the higher branches of the subject, and closely resembles, the matter treated on in Professor Nicholson's admirable 'Introduction to Biology.' But the method adopted by the two writers is far different. Mr. Wilson apparently aims at giving all sides of every controversial question, and of affording his students the opportunity of having every argument for and against all debateable points at their disposal. In fine, he pursues to a great extent the method adopted by Meckel and some of the old text-book writers, in giving all the arguments *pro* and *con.* on every moot point. We may cite as a characteristic example of the author's style his second chapter, on the nature of life and vital action. Briefly sketching the difficulties in dealing with the subject, he gives all the theories of vital action, accenting strongly those of Béclard, Dugès, Carpenter, Treviranus, Lawrence, and even Spencer. The author then states, "These definitions, as a whole, therefore, deal with the effects of vitality rather than with vitality itself. Such a result will, doubtless, be considered as inevitable in the present state of our knowledge; but we cannot avoid the fact that the knowledge we do possess does not warrant us in attempting to define or, what is much the same thing, to limit our ideas of the vital principle." He then wades into the whole controversy as to the nature and definitions of protoplasm or "bioplasm," and into the physical theory of life. The theory of a vital force is adequately discussed, as well as the subsidiary conditions for vitality, *i.e.* organization, air, temperature, light, and water. The part of this chapter bearing relation to "potential" vitality is most philosophical, and will well repay persual.

Other subjects are treated in the same exhaustive manner, the distinctions between the series of animals and plants; the comparison of their forms; motor power; chemical composition; intimate structure, and the nature and mode of assimilation of food. As on this topic the widest range of opinions have been expressed by zoologists, from Professor Owen and Mr. Hogg on the one hand to Professor Reay Greene on the other, the summary of the arguments on either hand adduced by Mr. Wilson will be of the highest value.

Stepping deeper into his subject, he boldly faces the esoteric problems of morphology, teleology, and the doctrine of animal "types." Dividing the animal kingdom into six sub-kingdoms—Protozoa, Coelenterata, Echinozoa (= Annuloida), Annulosa, Mollusca, and Vertebrata—he gives examples of each type. Whether these examples are the best chosen ones must depend on our idea of

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<sup>1</sup> *The Student's Guide to Zoology: a Manual of the Principles of Zoological Science.* By ANDREW WILSON. 1874.



the value of the classification itself. Whether a quintuple division would not have been preferable to a sixfold one is a question which we might argue at leisure with Mr. Wilson, but there can be no doubt of the ability and erudition with which he has marshalled his forces and made the most of them.

The division of the work on taxonomy or classification is naturally one of the best. The author's ideas of species are apparently of the most elastic and liberal order, and he well points out that in certain species the existence of so-called "dimorphic" or "trimorphic" forms—that is, forms included in a single "species," but exhibiting differences among themselves of insufficient value to constitute "varieties"—has but rendered the task of accurately limiting or defining such "species" all the more hopeless and confusing. We think, however, that he presumes too much on the authority and opinion of Mr. Darwin when he hints that the limits are known to zoologists under which permanent varieties are formed. This appears to be the case only in those animals, as the horse and pigeon, in which the variation is produced directly under the influence of man. But the problem how varieties arise in a state of nature appears, to us at least, almost to be insoluble, and its solution must depend on a far broader series of inductions than science at present possesses. Probably the boldest guesses and the most seductive theory is that which Mr. T. Belt has recently offered with regard to the changes observable in certain tropical insects, those of Nicaragua being especially selected for examination; yet Mr. Belt's arguments, although very ingenious, are far from being conclusive. Mr. Wilson, when giving a complete and exhaustive summary of the Darwinian theory, gives also due weight to the opinions of the numerous objectors, and he well sums up:

"Whilst thus the theories of the evolutionary origin of species endeavour each to reconcile to its particular ideas the probable causes and conditions which prompted and directed the existing order and relations of living forms, we observe that not one is exempt from objections more or less grave to its reception *in toto*. Yet from each we may derive a certain amount of information, from some a greater, and from others a less degree; and so far from such labours being discouraged as is occasionally inculcated upon us, it is clearly our duty to aid in all efforts which tend to the elucidation of the causes and means whereby this wondrous universe and its varied population have been derived, formed, and arranged."

Reversing the ordinary procedure, and stepping from the unknown to the known, and from the speculative to the actual, Mr. Wilson gives a description of the present state of our knowledge of histology, which in the hands of a student will bring him fully *au courant* with the modern level of this difficult and mysterious subject. Clear views are given of the various and divergent theories

of Schwann, Goodsir, Huxley, Bennett, and Beale. He is himself in favour of the idea of a "molecular bioplasm," the bioplastic matter serving as the primary medium and basis through which the molecules, as the more active centres of vital activity, evince their tendency to evolve higher structures and parts. This condition of a "molecular bioplasm" will also serve to account for those conditions, hitherto inexplicable by either theory alone, in which molecular activity seems to take origin and keep pace with the bioplastic or protoplasmic development.

It may be expected that the author should devote considerable space to the subject of heterogenesis, and that a summary should be given of the researches of Pouchet, Pasteur, Bastian, and Burdon Sanderson. We have already entered into this topic at great length in our pages, and here it will merely suffice to say that Mr. Wilson has admirably summarised the whole controversy. His authority leans decidedly to the party opposed to the opinions of Professor Bastian. We think, however, that his criticism of some of Professor Bastian's facts rests rather on the argument from their prior unlikelihood than their positive disproof from experiment. The latter method can alone decide the question, which has not yet reached its final stage, and the question of biogenesis or abiogenesis will probably have to be solved by a future generation of students.

It is surprising how the general laws of the geographical distribution of animals rest in precisely the same state as when, twenty years ago, they were laid down by the late Edward Forbes. Since his death, with the exception of the researches of S. P. Woodward and Sclater, our knowledge of the subject has rather become more clouded. Recent investigations into deep-sea forms, although accurate enough separately, are, when we view them as a whole, contradictory in their conclusions. Still, Mr. Wilson has well epitomised the little we know which is certain.

On the whole, he has written a pleasant and readable book, which may be safely placed in the hands of advanced students of zoology. We certainly would not recommend its study except to those who have already attained an intimate knowledge of particular forms. The illustrations are clear, and the whole work is elegant and compact.

### Recent Works on Chemistry.

*Armstrong's Organic Chemistry.*<sup>1</sup>—This compact volume presents a good view of the condition of modern organic chemistry. The plan of the book is decidedly philosophical, while the manner in which this plan has been developed shows that Professor Armstrong possesses great skill in selecting and arranging the immense accumulation of facts which constitute the history of carbon-

<sup>1</sup> *Introduction to the Study of Organic Chemistry.* By H. E. ARMSTRONG. Pp. i and 349. London, 1874.



compounds. Thus, we have here something very different from a mere descriptive catalogue of those organic substances which are best known and most important. The author's chief object is to illustrate the genesis, the chemical structure, and the relation of carbon-compounds by means of those substances best adapted to elucidate such points. On this account he has omitted all mention of those compounds the mechanism of which has not yet been discovered. We should search in vain in these pages for the familiar names of fibrin, casein, albumen, ossein, and gelatin, though in the exclusion of these substances from his pages we are not quite sure that Professor Armstrong is right. It would have been as well to have introduced a few words concerning bodies of such great importance and interest as these, even if the introduction had necessitated a confession of our ignorance as to their constitution. Then, too, if such compounds as the above be excluded from the volume under review, why is the very obscure and far less important group of pectose substances noticed? Indeed, when we examine minutely into the nature of the compounds described in these pages, we see that the special lines of Professor Armstrong's own researches have led him to develop certain portions of his subject to an extent which has prevented him from doing equal justice to other portions. And we may take this opportunity of demurring to some inaccuracies of expression and of statements which occasionally appear in the present volume. Starch-granules cannot be truly said to be made up (p. 190) of a series of "rings;" cellulose and lignin (p. 125) are by no means convertible terms; and phenol is more soluble in water than the expression "sparingly soluble" (p. 167) would seem to imply. These little matters are really of small moment, but the volume contains altogether rather a large number of such imperfections, as well as a good many dubious and awkward grammatical, or rather ungrammatical, constructions, which detract somewhat from the pleasure we have in reading a book otherwise so valuable.

It now remains for us to give some idea of the plan which has been pursued in arranging the materials of this text-book. The first of the twelve chapters introduces a definition of organic chemistry, a description of the methods of organic analysis, and the modes of ascertaining and controlling the formulæ, empirical and rational, of carbon-compounds.

In the next chapter the classification of organic compounds is given, the following order being adopted:—Hydrocarbons, alcohols, ethers, aldehydes, ketones, acids, anhydrides, amines, organo-metallic compounds. There are some great advantages in this grouping together bodies of similar function and structure, instead of arranging them in series according to the number of carbon atoms which they are presumed to contain. It is scarcely a forced analogy

which we suggest in comparing the former mode of grouping compounds to the natural system, and the latter to the Linnæan system of botany.

The next chapter (III) is devoted to a consideration of those processes of more or less general application by which carbon-compounds are produced; and then in the nine remaining sections of the book we have a systematic account of these bodies, arranged in the order which we have previously sketched. The bodies of the simplest structure are described first throughout the volume, so that in Chapter IV we have an account of the oxides, chlorides, and sulphides of carbon, and of other related bodies, which are often spoken of as inorganic. So under hydrocarbons the marsh gas series is first discussed, and then other series of more complex character. In the chapter on alcohols ethylic alcohol comes first, and the cellulosic and glucosic alcohols last; a similar arrangement is followed with the other groups.

The author has an evident mastery of his subject, while he brings down his information to the very date of publication. But the chief merit of the volume consists, after all, in the clear, connected, and well-ordered views which the author presents to his readers concerning the intimate constitution and relations of carbon-compounds. On this ground we have particular pleasure in commending Professor Armstrong's 'Introduction to Organic Chemistry.'

*Wanklyn's Milk Analysis.*<sup>1</sup>—This is a most disappointing book. In spite of the promises which the author makes in his preface, we cannot find that he has described a single improved method of examining milk. One would almost imagine that he had never read any analytical work dealing with the analysis of foods and of the products of the farm. Indeed, a student might infer that milk had never been properly analysed till Mr. Wanklyn accomplished the feat in the year 1871. That treasury of knowledge on the subject of milk which, under the title 'Du Lait,' was published in 1857 by MM. Guevenne and Bouchardat, appears quite unknown to Mr. Wanklyn; he has also ignored the subsequent experiments of Dr. Voelcker, which in one or two particulars have added to our knowledge of this subject.

The first serious mistake which our author makes is with reference to the question of the constancy in composition of cow's milk. Contrary to his statement (p. 6), cow's milk shows considerable variations, not only in the percentage of total solids, but in the proportions which the several constituents of those solids bear to

<sup>1</sup> *Milk Analysis: a Practical Treatise.* By J. A. WANKLYN. Pp. vi and 70. London, 1874.



each other. In point of fact, the evening milk is not identical with the morning milk; the milk of cows feeding upon a poor and overstocked pasture has been found to contain 9 per cent. less fat than that of the same animals feeding on good meadow herbage, though the other solids of the milk are not proportionately reduced; while, moreover, a moderate addition of oilcake to the ordinary food of a cow enriches the milk-solids by 1 per cent. or more. On the other hand, as the experience of some American cities proves too well, an abundance of washy and watery food does dilute and seriously alter the milk secreted by the cow.

In Chapter II the author speaks of the lactometer and creamometer in terms which most persons conversant with milk analysis will endorse. He condemns, however, the lactoscope, a useful instrument with which he confesses himself unfamiliar. Chapter III describes a mode of taking milk-residues, which is essentially the same as that generally used by chemists, though the quantities dealt with are rather smaller, and coagulation and stirring of the sample taken are avoided. A comparative study of the process employed by agricultural analysts and of that used by Mr. Wanklyn shows no real advantage in the latter, and, at all events, one drawback, for if we take, as he recommends, but five grains of milk in estimating the solids present, we obtain an amount of residue too small to admit of subsequent quantitative analysis. The next chapter (IV.) treats of the mode of estimating the fat in milk-residues. Here the author recommends a process which is not only clumsy, costly, dangerous and ineffective, but really hardly feasible. We are told to pour ether upon the milk-residue contained in a small platinum dish, to heat the liquid to the boiling-point, and pour the ethereal solution on to a filter paper. Just imagine the result of boiling ether in a open dish! How much would reach the filter? Why, when ether is rectified with every precaution for the condensation of its vapour a considerable loss is unavoidable. No wonder Mr. Wanklyn recommends us not to be sparing of the ether. The simple and effective apparatus for determinations of oil and fat, in which we distil ether through the material to be analysed into a flask, and then distil it back again, repeating this operation three or four times until the removal of oil from the substance is complete, appears to be unknown to the author.

It would be tedious to pursue our examination of this pamphlet further. We find the remaining chapters always imperfect and sometimes inaccurate. It is a pity that Mr. Wanklyn has not provided better directions for the guidance of the analysts appointed under the Adulteration Act, a class of men often in sore need of sound instruction.

*Rodwell's Birth of Chemistry*.<sup>1</sup>—This book may be regarded as a very attractive historical introduction to chemical science. It will, indeed, be read with considerable interest by students who have mastered the elements of modern chemistry, but we think that one useful purpose which it will serve is that of securing the attention of those persons who have not yet commenced the study of this science.

Mr. Rodwell has evidently devoted a vast amount of time and of patience in amassing, and afterwards in selecting and condensing, all kinds of details concerning early explanations of those phenomena and processes with which the modern science of chemistry is now concerned. He gives us some account of the origin of its name, and he discusses the neglect and contempt shown by the ancient Greeks for natural knowledge; and describes the various early views as to the origin and constitution of matter. Then follow two of the most interesting and instructive chapters in the book before us, comprising a condensed account of the practical chemistry of the ancients, as shown by their knowledge of the working of metals and of the manufacture of pigments and glass. Afterwards we are introduced gradually to the mysteries of alchemy, commencing with the association of the seven metals with the seven greater heavenly bodies, and the consequent introduction of symbols into the history of matter. Some fifty pages are next devoted to the alchemists and their writings, while the volume concludes with an account of the early experiments in pneumatic chemistry and the early theories of combustion and respiration. The book is agreeably written and well illustrated by means of a series of curious woodcuts, chiefly drawn from ancient monuments or manuscripts, and relating to metallurgy and alchemy. We wish Mr. Rodwell would give us a second volume, narrating in a similar way those great discoveries of the latter part of the eighteenth and first part of the nineteenth century which form the real foundations of chemical science.

*Schorlemmer's Chemistry of Carbon-Compounds*.<sup>2</sup>—Had this volume been published in a less luxurious and costly form, it would doubtless have formed an acceptable manual of organic chemistry for more advanced students of this science. But with its wide margins, its extravagant 'lead'-ing and 'space'-ing, and its high price, it does not compare favorably with Fowne's manual, which at a smaller cost provides the student with a concise account of chemical physics and inorganic chemistry, and treats organic chemistry with nearly the same fulness as Mr. Schorlemmer's work.

<sup>1</sup> *The Birth of Chemistry*. By G. F. RODWELL. Pp. xii and 135. London, 1874.

<sup>2</sup> *A Manual of the Chemistry of the Carbon-Compounds*. By C. SCHORLEMMER, F.R.S. Pp. xii and 512. London, 1874.



But it would be unjust to the author of the work before us, were we not to indicate its special features both of arrangement and of material.

The first fifty pages are occupied by introductory matter relating to the nature and functions of carbon in compounds, to the ultimate analysis of carbon-compounds, to the modes of determining molecular, empirical, and rational formulæ, and to the physical properties of the carbon-compounds. This part of the volume is clearly and concisely written. The systematic description of organic compounds occupies the whole remainder of the book, the order in which the various series of organic bodies are discussed being quite different from that of Mr. Armstrong's recently published work. Mr. Schorlemmer commences, it is true, with the simpler combinations of carbon with nitrogen, with oxygen, and with sulphur; but afterwards, instead of grouping alcohols together, acids together, and so forth, he arranges his subjects according to their complexity somewhat in the following order:—(1) Paraffins; (2) compounds of monad radicals; (3) compounds of dyad radicals; (4, 5, 6,) compounds of triad, tetrad and hexad radicals; (7) carbo-hydrates; (8) terpenes and camphors; (9) aromatic compounds; 10 glucosides; (11) artificial bases; (12) natural bases; (13) colouring and bitter principles; (14) biliary and other animal secretions; (15) albuminoids. In all these sections of his subject Mr. Schorlemmer gives us accurate information, noticing, wherever possible, not only the sources and outward characters of the subjects of his descriptions, but also the chief points which have been learnt concerning their chemical structure. There is so much merit in our author's treatment of individual groups and series of organic bodies that we cannot but regret that his classification should lack clearness, and that he should have entirely neglected to divide his book into sections or chapters, or to give us running titles. The presence of a full index goes some way towards remedying this defect.

*Cooke's New Chemistry*.<sup>1</sup>—Just as Mr. Rodwell's 'Birth of Chemistry' gives us in a popular and readable form the first crude guesses of philosophers as to the nature of matter, and then brings the story down just to the great period of chemical discovery which marked the latter part of the eighteenth century, so Professor Cooke's volume tells us, concisely and clearly, of the latest developments of chemical doctrine. If any one has learned his chemistry thirty or forty years ago, according to the school of Brande and Turner, he will find in the volume before us the means of remodelling his knowledge, so as to bring it in accord with the latest conclusions of modern research. Out of thirteen

<sup>1</sup> *The New Chemistry*. By J. P. COOKE, of Harvard University. Pp. 326. London, 1874.

chapters three are devoted to the consideration of molecules, three to chemical composition as expressed by the theory of atoms, two to chemical changes, reactions, and formulæ, and two to oxygen and the theory of combustion. Every one of these chapters may be read with satisfaction even by those who have mastered the intricate details of modern chemistry, but they will prove peculiarly valuable to those who desire to obtain an insight into its philosophical groundwork. In the last 100 pages of Professor Cooke's volume we have, first of all, a satisfactory description of what chemists mean by the "bonds" of an element, the term "quantivalence" being here very properly used instead of "atomicity" to express this idea; the word *vinaculance*, a simple translation of *bondedness*, is perhaps preferable. Then follow accounts of graphic symbols, of the electro-chemical theory of radicals, and of synthetical processes for the formation of carbon-compounds. Professor Cooke, in order to illustrate these subjects, selects a few appropriate examples, both from mineral and organic chemistry. These illustrations, even if not very numerous, are certainly well chosen, while every detail in them is developed with praiseworthy minuteness. In this way the reader sees exactly what he was meant to see, without being burdened by the introduction of collateral matter of minor importance.

*Church's Laboratory Guide.*<sup>1</sup>—The present edition of this useful manual of analytical chemistry as applied to agriculture has been enlarged by the addition of some fifty pages, and, as will be seen on comparing the tables of contents in the present and in the former edition, certain portions have been rearranged. The general methods laid down for the analysis of bone-ashes, superphosphates, coprolites, &c., are unaltered. The "fusion method" is recommended for the determination of phosphoric acid in Sombrero and Redonda guanos, as well as in certain nitrogenous guanos, such as those of Guanape, Ballestas, &c. In speaking of "reduced" phosphates the author disputes the theory that they are equal in value to those which remain soluble. His reasoning agrees with what has been repeatedly advanced by experienced and judicious agriculturists and manure-makers. Even though all soluble phosphates do ultimately become reduced in the soil, yet, by being at first presented in a soluble state, they possess an "initial diffusive power" which reduced phosphates lack. The former consequently are distributed through the soil more equally, and the chances are greater that every rootlet of the crop will be able

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<sup>1</sup> *The Laboratory Guide; a Manual of Practical Chemistry, specially Arranged for Agricultural Students.* By A. H. CHURCH, Professor of Chemistry in the Agricultural College, Cirencester. Third edition, enlarged and revised. London, 1874.



to find a particle of phosphate of lime in immediate contact with its pores. In estimating the "soluble phosphate" in superphosphates, the student is very properly cautioned against extracting with hot water. In fact, lixiviation with small successive portions of cold water is preferable to treatment at once with a larger volume.

To students of agricultural chemistry, and, indeed, to all interested in the knowledge of soils, foods, waters, and manures, we cordially recommend this new edition of Professor Church's 'Laboratory Guide.'

*Thorpe's Qualitative Analysis*.<sup>1</sup>—We have spoken (in a former number of this Review) so favourably of the 'Quantitative Analysis' of Mr. Thorpe, that we are sorry not to be able to accord as much praise to the present volume. In attempting to compress into some 200 small pages, not only chemical manipulation, but systematic qualitative analysis, both subjects suffer. This need not have been the case if the author had been content with a scheme of qualitative analysis less comprehensive in plan than that which he has adopted. The inclusion of chapters on the rare elements, on the detection of poisons, and on the examination of urine and urinary calculi, has seriously shortened the treatment which other and more elementary matters receive in the present volume. It is true, however, that these chapters are, as far as they go, very good. If the author have the opportunity it would be a good plan either to divide the book or to expand both sections of it, where expansion is needed, for the sake of clearness or the successful carrying out of methods. We would recommend doubling the number of early lessons on manipulation, and the introduction of better woodcuts and more practicable apparatus. It would also be advisable to prefix to each lesson a list of the special bits of apparatus and of the particular tests required for the performance of the experiments described. On the whole, we think that this little book may be made, with some pains, to develop into a concise and adequate handbook for the laboratory, useful alike to the candidate for the Practical Chemistry Examination of the Science and Art Department and for the student of medicine.

**West's Diseases of Infancy**.<sup>2</sup>—The appearance of a sixth edition may be taken to be pretty clear evidence that the critic's occupation in reference to the fortunate publication is gone; that whatever be his opinion the verdict of the reading public is distinctly pronounced in its favour, and that if he should perversely cherish an opposite

<sup>1</sup> *Qualitative Chemical Analysis and Laboratory Practice*. By J. E. THORPE and M. M. P. MUIR. Pp. xv, and 238. London, 1874.

<sup>2</sup> *Lectures on the Diseases of Infancy and Childhood*. By CHARLES WEST, M.D., &c. Sixth edition, revised and enlarged. London, 1874.

opinion, he should think once, twice, nay thrice, before expressing it, modestly surrendering individual to general conviction. Fortunately for the mental repose of the critic, no such surrender is required of him in connection with this new edition of Dr. West's book on 'The Diseases of Infancy and Childhood.' He thinks with those who by their patronage of the work consider it a truly valuable production. In every chapter of this new edition there are indications that Dr. West has endeavoured to set before his readers, not only the results of his evergrowing and mature experience, but also the conclusions arrived at by the most eminent men who have made an especial study of the diseases of infancy and childhood. But besides this, Dr. West shows himself still to be a disciple seeking after truth; a learner ready to accept whatever recent experience and research put upon a firm basis, and equally ready to surrender opinions cherished of old when found antagonistic to later teachings.

For instance, he has materially modified the practice he formerly recommended in several acute diseases, and depletion and calomel figure less prominently among his therapeutical resources. Recognising his own departure from the lines of practice approved by him in former years, he is disposed to assign the transition to an alteration in the type of disease. This may be accepted as an explanation, but we think it rather more valuable by reason of its convenience, its facility of application, and its elasticity, than from any positive evidence of its truth. We do not deny variations in the type of disease, but we receive the fact as one capable of very loose acceptation and of very convenient elasticity. At the present time, although we have met with no apologists for a recent change of type, we are conscious of a certain degree of reaction against the animosity that pervaded the minds of physicians a few years since with regard to depletion and the use of mercury. There is and always will be a fashion in medicine; there is and always will be a prevailing hypothesis, and, from this or that cause, the mode of practice of the day will be esteemed the correct and scientific one; and the reason why it differs from a foregoing plan will be enwrapped by moderate and cautious practitioners in the convenient hypothesis, that the constitutions of the sick differed of old from those of the current time.

Now, Dr. West, whilst apologising for the partial abandonment of his creed in respect of the virtues of depletion and antiphlogistics, nevertheless stops within the lines drawn by those who have mocked at antiphlogistics and found the true principle of treatment in the Brunonian hypothesis, and in so doing he has, apparently unconsciously, taken the side of those who can see and do admit at the present day the value of depletion and of so-called antiphlogistic remedies.

This able physician, indeed, confesses himself unable to ignore



the lessons of his early experience. He cannot shut his eyes to the fact that his treatment of old by bleeding, mercury, and antimonials had a large success—one at least equal if not superior to the expectant and do-nothing treatment which has of late years made many converts.

He still leeches and rightly enough, we hold, in active cerebral congestion; he actively employs antiphlogistics in croup, remarking that though his opinions have been altered by means of “the changes in the epidemic constitution of disease which recent years have brought with them,” yet “with every allowance for these changes he still believes that a decided antiphlogistic treatment is indicated in almost all cases of idiopathic croup.” At the same time he is compelled to say that cases requiring depletion have not occurred to him in the proportion they formerly did. Of this fact an explanation is offered, derived from the altered circumstances of his practice. He further adds, “I have met with not a few instances of idiopathic laryngeal croup which, in the hands of younger practitioners who thought of nothing but diphtheria, were being plied with stimulants and perchloride of iron, and were saved by antimony, by emetics, and the use of mercurials” (p. 408). So with regard to acute bronchitis and pneumonia, if he is prepared to acquiesce in the popular idea of the small value of bleeding, antimonials and mercury, the surrender of opinion is very partial and limited, and those ancient favorites reappear in his directions for treatment as the legitimate adjuvantia. In short, Dr. West exhibits great sensitiveness lest he should be placed in the list of old-world physicians, left far behind in the arena by the performances and pretensions of a younger school of physic; and by this feeling has been betrayed to express needless doubts about his former practice, which neither its history, so far as recorded, nor the convictions he now holds, notwithstanding every effort to ignore and depreciate his experience, do really justify.

Dr. West has now followed in the wake of most writers on children's diseases in using the term tubercular meningitis as the equivalent generally of what was formerly described as acute hydrocephalus. His chapter on this malady is very complete, and besides the information given respecting the old remedies recommended, presents the author's experience with newly vaunted medicines, such as aconite and chloral. A section follows on a lesion not much understood nor often recognised, viz., thrombosis of the cerebral sinuses. Chronic hydrocephalus, as a matter of course, is very fully considered, and its association with rachitis remarked upon. In the treatment of epilepsy Dr. West assigns the highest value to bromide of potassium, and appraises at a very low estimate belladonna, a drug much commended by Trousseau and Brown-Séquard. But in his history of epilepsy we do not meet with a clear conception of the varieties of epilepsy, as revealed by its clinical history. The

disease is dealt with too decidedly as a definite lesion, or one single morbid entity.

Chorea receives a fair handling. The hypotheses connecting this nervous disorder with rheumatism and embolism about or in the corpora striata, as a pathological result, do not receive the support of Dr. West. From a large experience with the disease we must also concur with that physician in rejecting them as insufficient and very frequently inapplicable.

With respect to croup and diphtheria the author adheres to the opinion, much attacked of late, that they are clinically distinct diseases, and that the pathological tissue changes, so much insisted upon in the maintenance of the doctrine of their essential similarity, or identity, are inadequate to establish it. He has well tabulated the differential features of the two diseases, and urges that, in place of such distinctive characters, the apparent and assumed similarity of the anatomical changes observed between them is no more valid evidence of the identity between them than are the alterations in the womb,—like as those are in appearance, evidence of the identity of simple metritis and puerperal fever.

We will no further extend our remarks on the special teachings of Dr. West, but restrict them to making a decided recommendation of his exceedingly valuable volume. The matter is not only excellent, but is also conveyed in a pleasing easy style, due largely to its original composition as a course of lectures.

**Winslow's Manual of Lunacy.**<sup>1</sup>—This compilation is more full and varied in its matter than any like productions previously published. The writer has evidently diligently sought to bring together all available information relating to the laws enacted for the care and protection of lunatics in public and in private asylums, or living singly with friends or others. In the copious appendices he has printed the clauses of the Lunacy Acts of 1845 and of 1853, together with those of the Amendment Act of 1862. The bulk of the volume is made up of a sort of running commentary and analysis of those several acts, intended to make their purpose and their machinery more intelligible to the inquiring reader than the legal jargon in which the acts are drawn is calculated to do. Some chapters are interspersed by way of information respecting the history of lunacy legislation and the state of lunacy in the three divisions of the kingdom, as well as in several countries of Europe and in the United States of America. The history of lunacy legislation is a mere sketch of familiar facts, and the notes on the state of lunacy in the United Kingdom are simply a collection of extracts

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<sup>1</sup> *Manual of Lunacy: a Handbook relating to the Legal Care and Treatment of the Insane, &c.* By LYTTLETON S. WINSLOW, M.B., &c. With a preface by FORBES WINSLOW, M.D., D.C.L., &c. London, 1874.



from the reports of the Lunacy Commissioners. Much cannot be said for the notice of the laws regulating asylums and the detention of the insane in some foreign lands; although, from the dearth of information on these matters, English readers may be grateful for even the small amount supplied them in this volume.

Chapter XIX is occupied with "definitions and explanations of terms generally used to denote various forms of insanity," but cannot be regarded as a very successful one. The observation on acute mania to the effect that it is generally associated with congestion of the brain, or with meningitis or cerebritis, does not accord with our experience or with the definitions of mania commonly given.

The volume is introduced by a preface by the writer's much distinguished father, Dr. Forbes Winslow, who since its publication has deceased. We concur in the opinion expressed in the preface that it will be a "valuable book of reference to lawyers as well as to all persons associated with lunatic asylums, and interested in the legal care of the insane." At the same time we do not as fully recognise the gap in medico-psychological literature which this book is presumed to fill; for, of late years particularly, that kind of literature has been pretty fully represented, and we need only refer to Elmer's '*Practice of Lunacy*,' to the works of Glenn and Weightman on the laws affecting medical men, and to the recently noticed '*Handbook of Law and Lunacy*' by Dr. Sabben and Mr. Browne. These works, indeed, do not discuss all the subjects contained in Dr. Winslow's volume, but they do set forth sufficiently distinctly, and some of them very fully, the actual requirements of the lunacy laws; and we will presume to say that, so far as lawyers are concerned, they will prefer the treatises written by their legal brethren, particularly as they are more or less rich in information respecting the ruling of judges on disputed questions of interpretation and other points in the which the legal mind fondly revels.

**Sieveking on Life Assurance.**<sup>1</sup>—By producing this small treatise on life assurance, Dr. Sieveking has done a good work. Considering the immense capital now invested in life assurance, and the importance, socially and politically, of the subject, it seems somewhat strange that so little has been written upon it from the medical point of view. Actuaries have contributed numerous papers discussing the value of bills of mortality and examining the statistical results of assurance societies, and thus have endeavoured to arrive at as certain and fixed rules as the nature of their subject matter admits of. But excepting Dr. Brinton's small treatise (1856), a brochure by Dr. Stephen Ward (1857), both now out of print, and the chapter on life assurance in Dr. Taylor's '*Medical Juris-*

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<sup>1</sup> *The Medical Adviser in Life Assurance*. By EDWARD H. SIEVEKING, M.D., &c. London, 1874.

prudence,' we know of no contributions made by the profession in this country, notwithstanding the vast amount of medical facts that must have accumulated in the archives of the many insurance offices. On this ground alone, therefore, as supplying a recognised need, we should welcome the appearance of Dr. Sieveking's volume. But beyond this the work before us has merits of its own to commend it to our approbation. The author writes from extended experience, as the medical officer of an assurance society carrying on a very large business, and from a thorough acquaintance with the literature of his subject. The scope of his work will be best shown by noting the subjects he treats of in the several chapters composing it. They are:—"The normal man;" "The duties of the medical officer;" "Hereditary influences;" "The history of the individual;" "The insurer's liability to disease;" "The medico-legal aspects of life assurance;" together with an introduction to the history and principles of assurance, and an appendix tabulating the causes of death for twenty years as registered in England.

As Dr. Sieveking writes from ascertained facts and admitted principles almost entirely, and introduces few hypotheses, there is little in his book open to the strictures of the critic, although there are numerous valuable and useful facts and comments which well deserve to be made public. However, as the book is small and within the means of every medical practitioner to purchase for himself, it is unnecessary to occupy our pages with extensive extracts. We shall therefore restrict ourselves to making a few general and, in some measure, critical remarks that have occurred to us in perusing the volume.

Now, the chapter on "the normal man" seems to us far below what it should be in precision of statements and of inferences; but this, not from the author's fault so much as from his inability to make it better, owing to the imperfect materials at command. The general impression we have gathered from the chapter, in fact, is, that we need a series of careful investigations and experiments to be yet made before we can draw the portrait of a normal man as exhibited in his conformation, his vital activities, and his power of resisting death. Mr. Hutchinson, some twenty years ago, accomplished much good work, and a few others have co-operated in the same field of research, but a vast area yet remains to be explored. The results hitherto arrived at by the spirometer, the dynamometer, and other mechanical contrivances for estimating bodily power and function, require revision and correction, inasmuch as all such contrivances are open to considerable error; for, as every one knows, skill and knack in their employment vastly affect the results, and, we would add, let not the returns of the Registrar-General be quoted as evidence, except for some rough-and-ready general statements, because they are not adapted for accurate scientific purposes. The



chapter on the duties of the medical officer is perhaps practically the most important in the book. Every medical man will doubtless claim for himself the ability to examine lives proposed for insurance; but unluckily the experience of offices shows that, however able practitioners may be, they somehow or other often fail to make an examination and report their results in an intelligent and satisfactory manner. Dr. Sieveking points out the errors most commonly committed, and gives good counsel to avoid them. He likewise reviews the influence of various occupations, and also of bodily defects and pregnancy upon the value of life. Here again the author is in want of more precise information than he is able to offer. For instance, as to the supposed safeguard of female life furnished by the pregnant condition, no better evidence is adducible than popular impression and the returns of the Registrar-General. What these last are worth for the purpose may be judged of by the figures themselves alone, which represent that throughout England only thirty-five pregnant women died in 1871. Why, we are confident that as many, nay more pregnant women died in London alone, and that the only value of such statistics is to show their worthlessness. Deaths are recorded and to a small extent their actual causes, but a concurrent state like that of pregnancy will seldom be mentioned. Even Dr. Sieveking, who seems to believe in the official returns, appears startled at the revelation he has come upon, for he remarks he "should scarcely have been prepared for such complete immunity (on the part of pregnant women) from fatal diseases" as those returns indicate. It reminds us of the very parallel discovery made a few years since, to the great joy of advanced radicals and adversaries to land-owners, evolved from the census returns, viz., that the whole country of England was divided among some three or four thousand land-proprietors. This startling discovery, after being well used in platform oratory, was finally resolved into nothingness by some individuals who looked into the census returns for themselves, and clearly proved that the small number was attributable simply and wholly to the fewness of those who happened to be satisfied with the designation of landed proprietors, the vast majority of persons having returned themselves as pursuing this or that profession or calling. With respect to the influence of parturition upon the prospect of life we would suggest to Dr. Sieveking the consideration not only of the sort of labours and of the number of children, but also of the rapidity with which children have been born; for this last circumstance is well known to have a considerable influence upon the health and vigour of women.

The observations and the lessons conveyed on the subject of the "insurer's liability to disease" are very full and very excellent, and ought to be studied by all those who act as medical referees of life insurance offices. They are given in very plain and compre-

hensible language with scarcely an exception, but in the following passage (at p. 122) an exception does appear, and the sense is obscure. Speaking of liability to disease, Dr. Sieveking writes, "In the case of brain disease, as of other morbid conditions, an early manifestation of the scrofulous taint would diminish our estimate of the individual's liability; and therefore, although in the majority of instances all traces of scrofula have disappeared at the time the individual becomes a candidate for insurance, the fact of his having suffered from some lymphatic disorder in early life must not be overlooked."

Here must end our chapter of notes and comments on this very excellent treatise on life assurance.

**Journal of Anatomy and Physiology.**<sup>1</sup>—By the truly scientific workers in anatomy and physiology the publication of this journal must be highly appreciated. It is the only one in Great Britain which, by limiting itself to the publication of original and scientific papers recounting the experiments and observations of their authors, conducted with the view of advancing or confirming existing knowledge,—can stand on the same platform with the best scientific journals of Germany. Yet we should do an injustice to it did we convey the impression that its matter is interesting only to the purely scientific inquirer. Far otherwise; for it contains papers which must prove of importance to the practical physician; and which, if in no other ways, contribute to elucidate daily phenomena witnessed at the bedside, and to indicate the direction for future clinical observation. We may cite such papers as those of Dr. Galabin, on "the causes of the secondary waves in the pulse," of Dr. M. Foster, on "the effects of a rise of temperature on reflex actions," of Mr. A. H. Garrod, on "the law which regulates the frequency of the pulse," and of Dr. Ferrier, "experimental researches in cerebral physiology and pathology," published in continuation of the previous series, which attracted such great and deserved attention throughout the scientific world. Besides the special contributions from the band of distinguished anatomists and physiologists, whose names alone are a guarantee for the excellence of their work, there are valuable reports on the progress of anatomy, of physiology, and of pharmacology, which place their readers *au courant* with the advances made in those subjects in all parts of the world where science has votaries. The report on Pharmacology, by Dr. T. R. Fraser, does scarcely, in our opinion, deal with what is commonly known as pharmacology. We do not contest its value, but consider its purport would be much better represented by describing it as a "Report on the Physiological

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<sup>1</sup> *The Journal of Anatomy and Physiology.* Conducted by Prof. HUMPHRY and Prof. TURNER. Second Series, No. XIII. 1873.



action of medicinal and poisonous substances," the title heretofore applied to it in the journal.

**Wilson's Anatomist's Vade Mecum.**<sup>1</sup>—The character and merits of this work have, by lapse of years and successive editions, been accepted by the profession, particularly by each rising generation, as an indisputable fact, to be thankfully received and duly acknowledged by purchase. It is a book removed well-nigh beyond the sphere of the critic, and the only hope of cavil of the more daring of the critical race must be sought in the account given of matters of general and minute anatomy, not in the description of coarser structure. The weakness in those subjects lies in their not being written up to the present time, but this defect is of less moment, inasmuch as no real student would limit his reading to this vade mecum of anatomy, but extend it to acknowledged text-books of histology and physiology. Again, some weakness is to be found in the illustrations to the present edition. When Wilson's vade mecum first appeared its pre-eminence among anatomical books lay in the excellence of its woodcuts. The same cannot be said of the present edition; wood-engraving and graphic illustration are arts that have of late years wonderfully progressed, and the observer of the woodcuts in the present volume cannot fail to see that many of them are much worn, and the letters of reference very indistinct. In future editions this defect in illustration should receive attention. But after expending our critical efforts it remains to us to have to commend this latest edition of this well-known treatise to all learners of anatomy.

**Woods Smyth on the Bible and the Doctrine of Evolution.**<sup>2</sup>—A critical review of this work would carry us beyond the confines allotted us in this journal specially devoted to medicine. It is enough to say generally what its author aims at. Of himself we are persuaded that he is an earnest and honest religious man, intent on upholding the doctrines of Christianity generally held and the belief in the Bible as a revealed book. At the same time he is ready to throw overboard many popular traditions respecting the creation of the world and of man. But whilst contending for what he regards as the literal truth of the Bible cosmogony, he accepts also as stated in his own words, almost without qualification, the doctrine of evolution, and connects it with the Bible, as but the physical or natural side of its own doctrines, which it illustrates, extends, confirms. This is accomplished, not by accommodating each to the other, but by accepting both in their simplest and most

<sup>1</sup> *The Anatomist's Vade Mecum, a System of Human Anatomy.* By ERASMUS WILSON, F.R.S. Edited by G. BUCHANAN, M.D., assisted by H. E. CLARK. Ninth edition. London, 1873. Pp. 743.

<sup>2</sup> *The Bible and the Doctrine of Evolution, being a complete Synthesis of their Truth, and giving a sure Scientific Basis for the Doctrines of Scripture.* By W. WOODS SMYTH. London, 1873.

manifest sense; the evidence of which is, that the plain and obvious interpretation of Scripture is the one most congruous with the "principles of evolution."

To estimate fairly how the author has succeeded in his task would require a careful study of the entire book; for it would be easy to select opinions here and there in its pages which could be turned to account by the critic disposed to treat the production with levity, and to extract amusement by critical arts from its pages. We on our part desire to bespeak for it a fair hearing.

Before concluding our remarks on the volume we may state our impression to be that the author has not accomplished, according to the programme, his task without "accommodating each to the other," scriptural doctrines and the theory of evolution, nor without going beyond "the plain and obvious interpretation of Scripture," unless, indeed, it be in his power to convince his readers that his mode of interpreting Scripture is the only plain and obvious one, and that expositors and commentators at large have all erred and strayed, and thereby do differ from him. We cannot do otherwise than think too that his interpretations are at times mystic, and that very common texts are made illustrative of cosmogenic and other doctrines those who wrote them had no conception of. His interpretations remove mountains of difficulties, but we fear his faith will fail to remove the mountains of traditional beliefs antagonistic to his own, and persuade their upholders that he is not as unorthodox as the very heterodox teachers of evolution doctrines.

**Cannes and its Climate.**<sup>1</sup>—True to its title this little book affords but a sketch of the climate of Cannes, and of the sort of invalids to be benefited by it. The climate is warmer and drier than that of places less favorably situated, and is sheltered from the stormy blasts of the north. The sky is bright and serene when clouds and rain obscure and mar the atmosphere of less favoured places. Vegetation flourishes and is represented by more numerous species of plants, and by such as are rather denizens of more southern lands. Chest affections and pulmonary consumption are the diseases for which the locality is best adapted, and, in general, if you are an invalid you cannot do better than go to Cannes and need not be troubled about the journey, for railways make it an easy one. Such are the grand outlines of the book; and they are such also as we always look for in a book of the sort. We accept their general truth, and would commend Cannes as an excellent resort for many consumptives and sufferers from pulmonary affections; and to such as propose to go there we would add, get this little book, which will give you some notion of the kind of place you are going to, and of the sort of life you should lead when

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<sup>1</sup> *Sketch of Cannes and its Climate.* By TH. DE VALECOURT, M.D., &c. Second edition, enlarged. London, 1873.



there. Moreover, if you have faith in meteorological charts, you will find six of them here of the most comforting character, if only you make out the zig-zags they display and put a right interpretation thereon.

**Proceedings of Pathological Society of Philadelphia.**<sup>1</sup>—We are pleased to call the attention of our readers to the ‘*Proceedings of the Pathological Society of Philadelphia*,’ one volume of which has reached us. Judging from the contents of this volume the Philadelphians have reason to be proud of their Society. Its members are evidently earnestly at work, and in the records of what they have accomplished prove themselves worthy rivals of the Pathological Society of London.

In the lists of office-bearers and members we find the names of many physicians who are well known and highly esteemed in Europe by their contributions to theoretical and practical medicine. We may quote those of Gross, Meigs, Pepper, Hutchinson, Da Costa, Weir-Mitchell, J. H. Brinton, J. G. Richardson, and Hartshorne, as at once catching the eye, and without making any invidious distinction by naming them only.

The volume now before us contains notes of the transactions of the Society for the period extending from September, 1866, to the end of the year 1870. Its matter, as already intimated, is highly valuable and interesting; but the manner in which it is published will admit of great improvement. There is much want of a list of papers and of a subdivision into sections representing the several sessions of the Society; and likewise neither printing nor paper is up to the mark. We commend to the attention and imitation of those charged with the publication of the Society’s ‘*Proceedings*’ the style and mode of production of the volumes of *Transactions* of our English medical societies.

**Proceedings of Dublin Obstetrical Society.**<sup>2</sup>—The Dublin Medical School has always held the highest reputation in the obstetrical art, and has given unusual opportunities and encouragement to the practice of midwifery. It moreover appears that an association was formed as long back as 1838 among the practitioners in Dublin interested in gynæcology, under the name of the ‘*Dublin Obstetrical Society*,’ which consequently has the precedence in time of the London Society. It was re-constituted in 1862, and put on a larger and firmer basis; but still until the last session was content silently to carry on its operations without putting them forth before the world by the publication of a volume of ‘*Proceedings*.’ Now the Society makes bold to challenge public consideration and criticism; and, in our belief, it need not fear the result.

Among the writers of papers we encounter the well-known names

<sup>1</sup> *Proceedings of the Pathological Society of Philadelphia*, vol. iii.

<sup>2</sup> *Proceedings of the Dublin Obstetrical Society for Session 1872–73.*

of Johnston, Atthill, Kidd, Churchill, M'Clintock, and Madden, from whose contributions the reader may confidently anticipate instruction. Besides some important cases, Dr. Johnston contributes the "Fourth Clinical Report of the Rotunda Lying-in Hospital for the year 1872," which, as a record only of the vast obstetrical practice of that institution, possesses great value. Dr. Moore Madden furnishes two considerable papers, one "on the Constitutional Character and Treatment of the Diseases of Women connected with Chronic Inflammation of the Uterus;" the second, "On the Diagnosis and Treatment of Uterine Polypi." Dr. McClintock produces a valuable analysis, with comments, on "The Excessive Vomiting of Pregnancy;" Dr. Fleetwood Churchill a notice of several "Cases of Amenorrhœa from Congenital Malformation," and Dr. Lombe Atthill an essay "On Endo-Metritis," its characters and treatment. Other valuable papers appear which it is not necessary to enumerate, and besides such communications there are notes of specimens exhibited to the society, illustrative of malformations and of morbid growths and appearances. Among objects exhibited we must mention "an instrument for the purpose of facilitating the application of caustics to the interior of the uterus," by Dr. Atthill, which seems admirably adapted to its purpose. There is a woodcut showing the appearance of this instrument, and one of an "improved uterine irrigator" recommended by Dr. Moore Madden; but excepting these and a rough diagram of a uterine tumour, shown by Dr. Atthill, the cases and specimens referred to in the text of the general papers are not illustrated. This may be regarded as a defect in any volume in which descriptive particulars constitute the larger portion, for the examination of a well-executed drawing will often convey more, and this more accurately, to the mind than any amount of verbal description. We trust the Dublin Obstetrical Society will so flourish as to be encouraged to attempt in future volumes pictorial illustrations where required.

**Transactions of Pathological Society of London.**<sup>1</sup>—As the proceedings of the Pathological Society of London, like those of the other chief societies in the kingdom, are recorded in the weekly periodicals, there is little need for noting them in detail in this Review when they come before us collected in the annual volume. At the same time we are most pleased to place well before the profession the work accomplished by our great medical societies as placed before us in their yearly volumes of transactions. We especially value them as furnishing a basis for elaborating analytical and critical reviews and digests of the questions of the day touching pathology and practice, inasmuch as in the scientific papers read and published, in the cases

<sup>1</sup> *Transactions of the Pathological Society of London*, vol. xxiv. London, 1873.



put on record, and in the discussions reported, we have a true reflection of the current doctrines and accepted practice. No London medical society started with more vigour, and none, perhaps, has retained more after the same lapse of time, than the Pathological Society. We think this may be partly due to its character and constitution, which have kept profitless talk in check, for nothing is more dreary and tiresome to the genuine student than the generalities and platitudes in which some members of our 'learned societies' indulge—men fond of hearing their own voice on all occasions, vain and pretentious, generally few in number at meetings, yet sufficiently numerous to be a bore, and to disgust their fellow members and lead to their withdrawal or to cold neglect of the meetings. In the history of pathology this present volume will always be referred to as of special importance by reason of the prolonged and able discussion on the anatomical relations of pulmonary phthisis to tubercle of the lung. This discussion is very fully reported, and perhaps shows a greater divergence among the speakers in the use of terms than in actual opinion respecting the pathological question debated. The rest of the volume is, as usual, occupied with brief descriptions of morbid specimens, with comments, reports from committees of reference, a catalogue of objects presented at the meetings, and a list of members. In the way of illustration are numerous excellent plates showing the general appearance and the minute structure of many of the specimens exhibited and described.

**Transactions of London Obstetrical Society.**<sup>1</sup>—The Obstetrical Society of London manifests a vigorous vitality, and now, after thirteen years' existence, can record a growing number of members and flourishing finances, can point to well-attended meetings, and can produce an annual volume of 'Transactions' rich in rare and interesting cases, and duly representative of the current opinions respecting theoretical and practical gynæcology.

The present volume is up to the average, and would be valuable for its recorded cases alone, but it will possess an exceptional interest by reason of the report of Dr. Heywood Smith's case, and of the prolonged discussion that followed its reading, on the propriety of intra-uterine injections of the perchloride of iron in post-partum hæmorrhage. It was a warm discussion, and has been followed by a paper war, much to be regretted on account of the manner in which it has been waged. The printed record of the discussion at the Society will be of much importance in estimating the truth of the issues raised. The Fellows may be well satisfied with the amount and quality of the matter contributed to obstetrical science and practice in this present volume.

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<sup>1</sup> *Transactions of the Obstetrical Society of London* (vol. xv) for the Year 1873. London, 1874.

**Nouveau Dictionnaire de Medecine et de Chirurgie.**<sup>1</sup>—The steady publication of this vast work must be as satisfactory to its subscribers as it is praiseworthy and honorable to its general editor, Dr. Jaccoud, and to its enterprising publishers, Baillière and Son. This eighteenth thick volume we may assume to represent nearly the completion of one half of the whole undertaking, which, when complete, will be a monument of the enterprise of French publishers and of the industry and learning of its writers.

The most important articles in the volume before us are—that on inflammation, by M. Heurtaux; on hysteria, by M. Bernutz; on hypochondria, by Ach. Foville; on infanticide, by Tardieu; on inanition, by Lépine; on hydrotherapeutics, by Beni-Barde; and that on jaundice, by Jules Simon. Other articles of less length, but on sufficiently important subjects, are those on hypnotism, by M. Duval; on sterility, by Siredey; on indigestion, by Luton; on hydrophobia, by Labadie-Lagrave; on illusions, by Motet; and on dropsy, by Straus. M. Hardy contributes two articles on ichthyosis and impetigo. A critical examination of the several articles is out of the question, but from a general survey a high opinion may be pronounced of them generally.

The author of the article on inflammation might have been better acquainted than he shows himself to be with English researches; and in the articles on hydrophobia, dropsy, indigestion, illusions, and idiocy, we are disappointed with the mode of their treatment, both as to completeness and accuracy of information and opinion. The term hydrophobia is so loosely employed that no definite knowledge is gained respecting one or other morbid condition embraced within its scope. The essay on dropsy betrays the absence or the neglect of acquaintance with some of the most important teachings of modern pathology; and of the article on idiocy it is enough to say it is written by one without enthusiasm for his subject, and without faith in what can be done for the improvement of the unhappy victims of mental deficiency. The subject matter of the articles on hydrotherapeutics and on hypnotism will attract attention by reason of the scarcity of books upon it falling within the domain of legitimate medical publications.

**Meigs and Pepper on Diseases of Children.**<sup>2</sup>—This voluminous and comprehensive treatise on children's diseases is really an

<sup>1</sup> *Nouveau Dictionnaire de Médecine et de Chirurgie pratiques.* Tome xviii. Hyd.—Infl. Avec 43 figures intercalées dans le texte. Paris, 1874.

<sup>2</sup> *A Practical Treatise on the Diseases of Children.* By J. FORSYTH MEIGS, M.D., and WILLIAM PEPPER, M.D. Fifth edition, revised and enlarged. London, 1874.



American production, though now an English publisher has taken to it, trusting, doubtless, to see it occupy as prominent a place in the estimation of our countrymen as it does in the United States. Its authors are connected with the excellent general hospital, with the Children's Hospital, and with the University of Pennsylvania, at Philadelphia, and their names will be familiar to many of our readers, owing to their attention having been directed to former editions of the volume. Moreover, we may again repeat our high opinion of the work. Its writers have evidently aimed at completeness and exhaustive treatment; consequently we have a big book of a thousand pages, large pages too, and closely printed. Such a treatise is a formidable affair, calculated to deter a student from meddling with it, and only tolerated by the practitioner as a book of reference. The size is certainly an evil, and an evil unavoidable on the plan pursued of collecting and representing every fact connected with each of the diseases treated of, and every opinion expressed on their pathology and treatment by the best-known writers of the age. This character of composition represents too much, in our opinion, of the bookmaking habit; a habit, by the way, very common with American authors. At the same time we must do justice to Drs. Meigs and Pepper, and state that they are no mere copyists of the views of others, but speak from a large experience, and advance distinctly enough their own opinions in pathology and therapeutics. These opinions they illustrate by many cases of disease and by a copious supply of formulæ. Bloodletting, calomel, and antimony still figure among the remedies recommended in several acute diseases, among which are pneumonia and pleurisy; and, on the whole, in the matter of treatment, the authors pursue a very moderate course, justifying its propriety by an appeal to the results of their own experience.

Instead of a progressive enlargement of this treatise,—marked on the appearance of each new edition and following on the process of revision, it would be more pleasing to most of its readers to find that revision meant as much curtailment as enlargement. This desirable object we hope to see attained when a new edition is called for. Every attentive reader will perceive the scope afforded for applying the pruning-knife. For instance, the descriptions given of diseases, of their pathology and symptomatology, are much overloaded by matter applying to the selfsame lesions in adults; and, in like manner, it would appear that the authors, in order to be "full," have, instead of principally dealing with such details as are peculiar to infantile life, gone into questions of etiology, symptomatology, morbid anatomy, and of therapeutics, as though their business

was to say all that can be said of each lesion described. Whereas, on the contrary, writers of special treatises should bear in mind that their readers want the special knowledge such works presumably impart, and are already well acquainted with what is general.

Still, in spite of its unwieldy dimensions and its encumbrance with needless details, we hold to our expressed opinion of the value of this treatise to the practitioner as a book of reference.

**Braidwood on Management of Children.**<sup>1</sup>—This small work may be confidently recommended to those in whose interests it is written. Dr. Braidwood addresses those of the public who have charge of children, and the advice he gives them is very good and salutary. He has been induced to undertake the task of an instructor in the management of children, both from a conviction that his own experience enables him to assume the office, and that the attempts hitherto made by Combe and others in that direction have been too ambitious or too much expanded. At the same time he is ready to acknowledge the great value of Combe's maxims, and he seeks rather to reduce them within a smaller compass of printed matter than to replace them—to make a small, cheap book, accessible to all, rather than a more elaborate treatise addressed to the few.

**Ringer on Temperature in Phthisis.**<sup>2</sup>—The clinical value of this small treatise will be recognised by every one who peruses its pages. The elevation of temperature in phthisis is no recent discovery, but the knowledge of the conditions determining that elevation, and of the variations therein dependent upon different pathological states, is the result of modern investigations by the thermometer.

Dr. Ringer lays down eight propositions, which he proceeds to examine and illustrate *seriatim*, insisting upon the reliability of thermometric observations and their superiority in diagnosis and prognosis to the general signs and symptoms of phthisis. Under this general term he includes three varieties of lesion—viz. catarrhal or scrofulous pneumonia, true tubercle, and the fibroid lung, the leading features of each of which get illustrated, whilst he shows how an appeal to temperature will enable the observer to distinguish them from one another, and not only this, but also how it will serve to arrive at a conclusion

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<sup>1</sup> *The Domestic Management of Children.* By T. M. BRAIDWOOD, M.D. London, 1874.

<sup>2</sup> *On the Temperature of the Body as a means of Diagnosis and Prognosis in Phthisis.* By SIDNEY RINGER, M.D. Second edition. London, 1873.



respecting the stage of disease and thereby give greater certainty to prognosis.

Such is the general scope of Dr. Ringer's little book, which, as a brochure obtainable by all, calls for no analysis. Our duty is to recommend every practitioner to make himself thoroughly conversant with its teachings, as he will thereby find himself much advanced in accuracy of diagnosis and prognosis regarding the most common and fatal malady man is subject to.

**Ferraud's Vade-Mecum of Pharmacy.**<sup>1</sup>—This work appears well calculated to fulfil the purpose for which it was written. The amount of information it contains is very considerable, and the manner in which it is conveyed terse and clear. It presents an account of almost every drug to which medicinal virtues are attributed, of various substances required by the pharmacien and chemist, and, moreover, of matters more or less outside even the scope of pharmacy and materia medica—as, for instance, albumen, beer, wine, and urinary calculi. In the case of each article are detailed its derivation, its physical appearances, and chemical constitution, with instructions for its preparation; its physiological, therapeutical, and toxicological effects; its doses, and both its recognised and unrecognised or quack formulæ. As examples of the last, we may mention the composition of Holloway's and of Morrison's Pills, besides that of most of the very many secret remedies in repute in France. These may be regarded as curiosities of pharmacy; but over and above such, the formulæ of a large number of ingenious and elegant preparations for which French pharmacy is justly noted are described. These last are particularly worthy of the study of our pharmaceutical chemists (why not called more briefly and euphoniously, pharmaciens or pharmacists?); for English drug-making is assuredly yet open to immense improvements, and there is no need we should be compelled to swallow either the big doses or the nauseous preparations of even the improved British Pharmacopœia.

Most of the woodcuts in this volume might have been as well left out, for they are often too coarse and imperfect to aid the reader in apprehending the characters of the articles they are intended to illustrate. Altogether the volume has been carefully compiled, and may deservedly be recommended to all students of materia medica and pharmacy.

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<sup>1</sup> *Aide-Mémoire de Pharmacie: Vade-Mecum du Pharmacien*. Par EUSÈBE FERRAUD. Avec 184 figures. Paris, 1873.

**Bell's Manual of Surgical Operations.**<sup>1</sup>—A third edition of a book of this sort is a sufficient guarantee for its character and utility, and affords evidence that it is well appreciated. Mr. Bell writes from large experience, both as a surgeon and a lecturer on surgery, and may be assumed to be well acquainted both with the wants of students and junior practitioners, and with the most eligible way of imparting the information they require.

The book is really a manual in the proper sense of the word; the instruction given in it is simple and without extraneous matter, and the style in which it is written is clear and concise. Thirty-eight illustrations are given in elucidation of the description in the text. The chapters on amputations and excisions are preceded by short historical notices respecting the opinions held from time to time, and the varieties of practice introduced by various surgeons. Altogether it is a book that can be safely and strongly recommended to those for whose use it has been specially written.

**Curtis on Treatment of Stricture.**<sup>2</sup>—This small book on the treatment of stricture of the urethra by gradual dilatation is an essay to which the Civiale Prize for 1872 was awarded. We may, therefore, fairly expect to find it a work of merit. It goes over the whole subject of stricture, dealing chiefly with cases to which progressive dilatation is applicable; and we need scarcely tell our readers, that that includes by far the greater proportion. It indicates, but does not dwell upon, the rarer examples, in which some other operation, such as forcible rupture or over-distension, is required.

Mr. Curtis is evidently an admirer of the English school, and we note with pleasure that his opinions are formed in a great measure on the teaching of Sir Henry Thompson, and other of our best authorities. It is gratifying also to know, that such sound views of urethral disease and its treatment are so widely diffused.

The author is a strong advocate for the use of those soft bulbous bougies which we have derived from the French, and which have proved such a boon to the sufferers from stricture. It would be well, also, if we were to introduce the French gauge into our practice, for there can be no doubt that the gradations in size of our English instruments are much too

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<sup>1</sup> *A Manual of the Operations of Surgery, for the use of Senior Students, House-Surgeons, and Junior Practitioners.* Illustrated. By JOSEPH BELL, F.R.C.S. Edin., &c. Third edition, revised and enlarged. Edinburgh, 1874.

<sup>2</sup> *Du Traitement des Rétrécissements de l'urèthra par la dilatation progressive.* Par T. B. CURTIS (Boston, U.S.A.), M.D. Paris. Travail couronné par la commission du prix Civiale pour l'année 1872. Paris, 1873.



great; for the system of gradual dilatation, which it is the object of this monograph to recommend, can only be properly carried out by a series of instruments, whose calibre increases by very minute degrees.

Mr. Curtis has appended to his essay a table of seventy cases treated at the Hôpital Necker. Of the patients whose cases are included in this series, he gives the age, the number of the instrument which was passed when they were first seen, as well as of that which could be introduced when they were dismissed, and the duration of the treatment. From this table he concludes that the average duration of treatment by progressive dilatation is twenty-eight days, and that the stricture is dilated by the extent of a number every three days. Of course, this treatment does not effect a permanent cure. No method does this. It dilates the stricture to its full extent with the least pain and inconvenience to the patient; and if he will take the trouble to pass a full-sized instrument himself once a week he may keep himself practically in a cured condition. And here we may remark that the soft French bougie glides along the urethra so easily, and its introduction is so free from discomfort, that it is no difficult matter to persuade patients to pass them.

**Goulay on Stricture of the Urethra.**—This work professes to deal with the whole subject of urinary diseases; but this it does in very varying proportions. Some points are fully discussed, while others are passed over very lightly. Its chief fault is that it is too exclusively devoted to operative proceedings. A very distinguished London surgeon has lately drawn attention, in the pages of the 'British Medical Journal,' to the *medical* treatment of stricture of the urethra, and he has laid great stress upon the measures which may be adopted in order to relieve the patient, even before a catheter or a sound is passed. Perhaps Mr. Savory goes too far in this direction. But there can be no doubt that what may be called the medical treatment of stricture is too much overlooked, and that surgeons are apt to proceed to operations of greater or less severity before they have exhausted the milder measures by which the case may often be ameliorated, if not cured.

There may be something in American life which makes the class of cases that come before a surgeon differ somewhat from those which we usually see in this country. Severe accidents may be more frequent, and it may be more difficult in outlying

<sup>1</sup> *Diseases of the Urinary Organs; including Stricture of the Urethra, Affections of the Prostate, and Stone in the Bladder.* By JOHN W. S. GOULAY, M.D., late Professor of Surgery in the University of New York. New York, 1873. Pp. 368.

parts of the country to get good surgical advice promptly. But whatever may be the reason, we have been struck, while carefully perusing this volume, at the large number of cases that have been submitted to severe operations—a much larger proportion, we imagine, than would be found in the practice of the leading operating surgeons on this side of the Atlantic.

The author opens with a good account of the various modes of examining the urethra for stricture and of the instruments which are used for that purpose, the best of which, like the bulbous bougies, we owe to our French brethren. He then explains the method of using the *tunnelled sounds* which he has introduced, and which are among the most useful of the novel suggestions contained in the book. A very fine whalebone bougie is first insinuated gently through the stricture, and then the tunnelled sound is threaded upon it and passed over it into the bladder. Subsequently other larger sounds of the same description may be introduced, and thus the first step is taken towards the treatment of the stricture by gradual dilatation. In some instances of very narrow and tortuous strictures the whalebone bougie and the tunnelled sound may obviate the necessity for a severe operation. The space, however, which the author bestows upon these simple methods is but small, and he passes on at once to discuss divulsion, internal urethrotomy, and external urethrotomy. Upon this last subject he dilates at great length—at very disproportionate length, as it appears to us; for, after all, the milder measures which are applicable to the vast majority of cases are of more importance than the severe operations which are applicable to only a few. The subject of retention of urine next engages his attention, and here he reverts again to the use of his tunnelled sound, which would appear in cases of this kind to be sometimes of real value. Rupture of the bladder or urethra, with their terrible accompaniment, extravasation of urine, naturally follow next, and the subject is treated at considerable length and gives scope for the introduction of some interesting cases. Then follow chapters upon the diseases of the prostate, and the work concludes with the consideration of lithotomy and lithotripsy.

Thus it is clear that a great many most important subjects are traversed in the comparatively brief space of 368 pages. The great fault of the work, as before remarked, is that, while it proposes to treat of the whole subject of the diseases of the urinary organs, it really is principally occupied with their operative treatment. For example, cystitis, both in its acute and chronic forms, is a most troublesome and distressing affection, often taxing all the resources of the practitioner, and



yet it is here passed over in a couple of pages. It admits of no great operation; it requires much careful watching and much attention to details on the part of the surgeon, and hence it does not fall in with the genius of our author.

**Canty on the Skin.**<sup>1</sup>—This work consists of twenty-four letters addressed to an imaginary “Sir,” who may be supposed to be thirsting for instruction in cutaneous medicine. We learn from the author that the publication is much needed, inasmuch as dermatological literature is in an inchoate state; we are, indeed, invited to make merry over the shortcomings of some well-known English treatises, which, in their superficiality, verbiage and inaccuracy, seem as foils to the more philosophical work before us. But poor as our countrymen’s performances may be, our patriotism is gratified by learning that we are, nevertheless, in the van of progress. In combating the assertion that France and Germany are in advance of us, our author remarks—

“The three great additions to the means of cure of skin diseases have been glycerine introduced by Startin, calomel baths by Henry Lee, and the articles on arsenic and mercury by Hunt—all English. That other nations have written on dermatology, that they have examined, noted, and collected numerous cases and interminable minutiae, is very true, but the result of all such work is simply *nil*.”

It is for this reason, we presume, that not a single reference is made to any foreign dermatologist of eminence, except Hebra, whose work, as far as principles of treatment or causation are concerned, “might as well have been written by a machine.” As Englishmen, we may feel proud to have two authorities who can be appealed to, and *are* quoted as thoroughly trustworthy, viz. Mr. Hunt and Mr. Jabez Hogg.

Turning to the work itself, we learn that the author’s purpose is to unite skin affections to general medicine, and therefore much of the volume is occupied with the discussion of fundamental questions in pathology. Here we are met with much that is new and somewhat startling. “The blood is a fluid containing red granules, smaller indefinite granular matter, and white corpuscles.” “In disease the white corpuscles increase in number and the red granules decrease in number.” The performances of the white corpuscles are so remarkable that after reading this book we have come to the conclusion that nothing but a foreign incapacity for observation could have allowed Cohnheim to have passed over their mutations.

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<sup>1</sup> *Diseases of the Skin: in Twenty-four Letters on the Principles and Practice of Cutaneous Medicine.* By HENRY EVANS CANTY, Surgeon to the Liverpool Dispensary for Diseases of the Skin. Pp. 365. London, 1874.

“ We have four states in which the white corpuscles increase in number—1, with a quickened circulation, the blood being watery; 2, the blood containing solids in excess; 3, the blood with excess of solids, the circulation being retarded; 4, the latter state, with watery blood ” (p. 14).

This, at first, might seem as far removed from practice as the observations of the most subdividing German; but we soon learn that in the varying rapidity of the circulation and the vicissitudes of the white corpuscles, with an occasional change in the condition of the “ red granules,” lie all the deep secrets of pathology and therapeutics. What can be clearer or more beautiful than the following account of the changes caused by syphilis and the counterchanges wrought by mercury?

“ The exact action of the (syphilitic) germ, when absorbed, is to retard the circulation. You have then all the symptoms of depression, lowered pulse, absence of colour, dulness of the eyes, lassitude, &c.

“ The blood is rendered thin and watery, the red granules decrease in number, increase in size, and become soft, pliable, and easily mix with the fluids, losing their separate existence; the white corpuscles increase in size, and become softer in texture; as a consequence of this state of things, at some point or points of the tissue the enlarged white corpuscles become incapable of passing through the capillaries; you have then a point of stagnation around which both white corpuscles and disease germs increase and multiply; as a consequence of such increase of white corpuscles you have the various hypertrophies of scales and tubercles, or if there is exudation beneath the epidermis, papules, to all of which formations the syphilitic germ gives the characteristic sign of red granular matter mixed with the fluid exuded ” (p. 135).

“ The general irritant action of mercury produces a quickened circulation throughout all the vascular system, glandular or otherwise; the red granules increase in number, diminish in size, and are rendered firm and tough; the white corpuscles diminish in size and in number, any stagnant spots being removed; the glands, increased in activity, eliminate the metal and throw out more aqueous matter with their special secretion, and some of the disease germs escape with the secretions, whilst the further multiplication of such germs is arrested if only for a time ” (p. 163).

Other passages of equal splendour and fidelity to fact adorn the volume.

Mr. Canty has a stock of curiosities of experience. He finds a great prejudice amongst mothers and some practitioners against lancing the gums, even in serious cases. “ You may see a child lying perfectly insensible on its mother’s knee, *with twelve or fourteen teeth shining through the hardened gum*, and yet mother and attendant agreeing it is ‘not the teeth.’ ”



(p. 62.) “Erythema nodosum is an elevation of the skin into nodules, generally seen on the skins of young women” (p. 38.) A patient saturated with mercury becomes so insensible to the action of the drug that “a scruple of calomel twice a day produces little or no effect. . . . In these cases galvanic baths will soon render the patient amenable to mercurial influence. The success of these baths in so doing, and also in stopping mercurial action, is very marked; the only unsatisfactory part of the process is, that you are not able to detect the mercury in the liquid or on a negative pole” (p. 166). But the most remarkable case is one which occurred thirty years ago.

“A person treated with prussic acid in full doses until nearly killed by it recovered from the irritant action of hydrophobia, and the case lived long enough to develop symptoms resembling typhus. . . . As variola passed through a cow becomes vaccinia, so some form of typhus passed through a dog might become hydrophobia” (p. 217).

We have quoted freely, because we felt that any mere description of this volume would inevitably seem exaggerated. Our extracts fairly represent the sort of nonsense found on nearly every page. Throughout the book there is not a single accurate description of the appearance, course, histology or pathology of any one eruption; not a single definite indication for the employment of any remedy (with the exception of a reference to Hunt’s rules for administering arsenic); not a single intelligible record of a case, experiment, or observation. The whole work seems to have been evolved like the celebrated German camel. The style is on a par with the matter. Long involved sentences abound, pronouns and participles are strangely treated, relations agree with the most unexpected antecedents, and digressions are the rule and not the exception. We are treated to such words as *tenia*, *systematic* (for *systemic*), *punctæ*, *dubiousness*, and *scthematous*. The book is well printed, on good paper, well indexed, and handsomely got up; by these outward and visible signs only can it commend itself.

**Letievant on Nerve Sections.**<sup>1</sup>—This is a very complete and well-arranged treatise upon the section of nerves, which, from its clearness of expression and excellence of illustration, will be found a most useful guide in this branch of surgery. The book is conveniently divided into three parts:—

<sup>1</sup> *Traité des Sections Nerveuses.* Par E. LÉTIÉVANT, Chirurgien en chef désigné de l’Hôtel Dieu de Lyon, &c. Paris, 1873.

The first is a study of the pathological physiology of nerve sections in man; the second treats of the indications for the division of nerves in disease; and the third is devoted to the description of the mode of performing such operations.

Part I is largely concerned with the exposition of M. Létiévant's theory of the mode in which sensation and motion are produced in parts supplied by a divided nerve. He shows that even as soon as three hours after the division of a nerve the parts supplied by it are found to possess a certain amount of sensation and motion; and as it is impossible that regeneration of any part of the nerve could have so rapidly taken place, it is necessary to explain this in some other way. This M. Létiévant does by the theory of vicarious sensation and motion. That is to say, that the sensations that should be conveyed along the divided nerve now travel by other channels; and the movements depending upon the action of the muscle of the divided nerve are performed by combinations of other muscles acting in their stead.

Vicarious sensations depend upon the presence of nervous filaments from the paralysed part which anastomose with others from undivided nerves, and also upon the perception of impressions by papillæ in the neighbourhood of the paralysed part, and which are connected with uninjured nerves. That such anastomoses exist the author shows has been demonstrated by anatomists, and he argues that it is reasonable to suppose that their use is to fulfil, in part, the function of the nerve when it is injured. Moreover, cases are quoted to show that after the division of a nerve, although the irritation of the trunk above the point of section causes no effect, stimuli applied to the parts below that point are painfully felt. And to these facts he adds the confirmatory experiments upon animals of MM. Arloing and Tripier.

An illustration of the perception of sensations by the papillæ in the neighbourhood of the paralysed part is given by a case in which, after accidental division of the median nerve, no pain whatever was given by thrusting a pin into the index finger even to the bone, or by immersing the finger in water at a heat intolerable to the other fingers, but in which a slight scratching by a pin on the end of the paralysed finger was distinctly perceived, and the patient was even able to tell the position, force, and direction of the movement. The author found also that he could perceive such movements made upon the finger of another held between his own fingers. These perceptions M. Létiévant attributes to the appreciation by the papillæ of his own fingers of the stimulus which this movement produced upon them, even from some distance.



Concerning the vicarious movements, M. Létiévant shows that they cannot be performed by the muscles supplied by the divided nerve, for there is an absence of contraction of these muscles, and they become atrophied, and thus give rise to characteristic deformities; besides which there are evident departures from the ordinary movements of the limb. The movements must therefore be due to the vicarious action of other muscles supplied by uninjured nerves.

From a study of the effects of nerve section we may gather the following facts:—That, in spite of the division of a nerve, there always remains, in the parts it supplies, a certain amount of sensation, if it be a sensory nerve; of motion, if it be a motor nerve; or of both, if it be a mixed nerve. These functions, though considerably impaired, are carried on by the intervention of agents unconnected with the divided nerve, viz. muscles, anastomotic nervous branches, and the neighbouring nervous papillæ. These vicarious functions are at first very imperfectly performed, but, in proportion to the lapse of time from the section, they acquire, by use, greater development. This development coincides sometimes with a remarkable muscular atrophy. Thus, the vicarious motion coincides at first with a paralysis, and afterwards with an atrophy also, of the muscles supplied by the divided nerve; it never reaches, however, the normal perfection of movement. The vicarious sensation in the region supplied by a divided nerve is relatively more marked than the vicarious movement, but it also never reaches the normal standard.

In some cases the existence of this sensation may be masked by accidental conditions, as, *e. g.*, inflammatory swelling or (especially in the case of nerve sections for neuralgia) a sort of local stupor affecting the part for a short time. The vicarious functions are of variable duration—temporary in some cases, permanent in others. Recovery after nerve section is sometimes complete, and is characterised by a restoration of function and muscular nutrition consequent upon the regeneration of the nerve; but this recovery occurs only after the lapse of considerable periods (twelve to fifteen months).

The second section of the book concerns the indications for nerve sections in disease. For neuralgia the author strongly advocates the division of nerves, and asserts that for this disease success is the rule, failure the exception. He says that neuralgias of peripheral are more common than those of central origin, and that these are generally benefited by neurotomy; for if the irritating cause cannot be removed, division of the nerve which transmits the impression will extinguish the neuralgia. We think this is by no means always

the case; for although the cause may have been originally peripheral, there may have been such an impression made upon the centres that even the removal of the cause leaves the effect still remaining.

The author considers, however, that neuralgias even of central origin may sometimes be relieved by neurotomy, which prevents the secondary effects of motor irritation. He points out that neurotomy for neuralgias originating in inflammatory or organic affections of nerves (as, *e. g.*, in neuralgias of stumps) should be made well above the point affected, or there will be the danger of operating upon a diseased part of the nerve—a frequent cause of failure. For the relief of the pain of cancers and ulcers the operation is, no doubt, justifiable; but we have, as yet, but little experience in this direction.

For traumatic tetanus M. Létievant argues that neurotomy may often be advantageously performed; and he quotes sixteen cases, in ten of which the operation was followed by success. An important guide (pointed out by Mr. Wood) to the nerve which should be divided is that pressure upon it causes pain, which is referred to the wound. The section should be made at a part removed from the source of irritation.

The performance of neurotomy does not, of course, exclude the use of other remedies.

Five cases of neurotomy for epilepsy, apparently of peripheral origin, are given, in four of which it was followed by recovery. A case of chorea, probably depending upon a neuroma of the internal plantar nerve, is quoted, in which the division of this nerve led to a cure.

Some spasmodic contractions due to peripheral excitation have also been cured by neurotomy. M. Létievant also shows that neurotomy may sometimes be resorted to with benefit in cases of tumours of nerves wherein the tumours cannot be removed. Finally, it is suggested that phlegmonous inflammation and glaucoma may be influenced beneficially by neurotomy.

The third section of the work is devoted to the description of the various operations alluded to in the preceding sections, and contains precise directions for finding the different nerves. The descriptions are illustrated by appropriate woodcuts.

**Surgical Inquiries.**<sup>1</sup>—The contents of this volume were delivered as a presidential address at the annual meeting of the Birmingham and Midland Counties Branch of the British Medical Association. They consist of short observations on subjects in which the author has taken interest. They are extremely brief

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<sup>1</sup> *Surgical Inquiries.* By FURNEAUX JORDAN, F.R.C.S. London, 1873.



—in fact, little more than notes. “At another time,” Mr. Jordan tells us, “he may see fit to expand them in some degree.” We sincerely hope he will, for in their present form many of them are valueless, as being mere statements of Mr. Jordan’s opinions, unsupported by any scientific evidence. Thus, in the paragraph on skin-grafting he tells us, “new epithelium may arise by proliferation from the old epithelial cells of the transplanted graft—it cannot be made out of lymph.” Now, this is very easy to assume, but definite proof is very difficult, as may be seen by reference to the article on this subject in Dumarquay’s work on regeneration. A little further on he tells us that “old inflammations, sinuses, cicatrices, ulcers, carious bone, *often* go straight on to cancer.” If he had accurately recorded one case in which carious bone had become cancerous it would have been of more value than any number of such unsupported assertions.

On struma he tells us that he has “long felt that struma is simply transmitted syphilis,” and that he “cannot but think that if there had been no syphilis in the world there would have been no consumptives, or angularly curved spines, or so many crippled joints.” We might remind him that consumption exists abundantly in the lower animals, among which syphilis is unknown. Mercury, he acknowledges, cures hereditary syphilis. Does he find it equally beneficial in struma? Mr. Jordan unhesitatingly affirms that he has never seen varicose veins give rise to an ulcer without first producing eczema. No doubt the combination is very frequent, but is it constant? He has also never seen a case of “Brodie’s or so-called synovial gelatinous disease” in which “chronic bone disease was not present, and had existed before the synovial complication.” Again, in cases of hæmorrhage from a ruptured meningeal artery he recommends us to ligature the common carotid on the affected side instead of trephining. We need not quote further. The address seems to us out of keeping with the spirit of the times. The day of dogma is gone by, and the opinions of even so accomplished and thoughtful a surgeon as Mr. Furneaux Jordan will not be received as accepted facts unless they are supported by evidence.

**Hygiene of Schools.**<sup>1</sup>—The intention of the author of this little book is good, for there can be no doubt that with all our advanced knowledge, the mental and physical training of the young is by no means wholly satisfactory in the present day. But the execution

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<sup>1</sup> *The Hygiene of Schools, or Education Mentally and Physically Considered.*  
By J. B. BUDGETT, M.D. Pp. 88. London, 1874.

of Dr. Budgett's task is not equal to the design, and he displays no small amount of carelessness on points where accurate information ought to be conveyed to the general public. For instance, he states, as a fact, that on the 9th of December in last year, when a remarkable fog prevailed in London, the proportion of carbonic acid in the atmosphere had increased from its normal proportion to 5 *and in some localities to 8 in 100 part (!!).* Dr. Budgett calls this "a reasonable explanation" of the mortality then observed from diseases of the respiratory organs in the metropolis, and of the loss of valuable animals at the Cattle Show, and indeed if there had been such a proportion of carbonic acid in the atmosphere, we apprehend that none of the inhabitants of London would have survived to tell the tale. The remarks on the important subject of food are very superficial, being, we should imagine, utterly useless to the general reader, and certainly imperfect in a scientific point of view. Proteids, Dr. Budgett tells us, correctly enough, are gluten of wheat, albumen, and caseine of cheese; but he says not a word of meat as a proteid, and we are unable to understand his meaning when he tells us (p. 63) that nitrogenised food is not so much required nor so easily digested by young children as by adults, "*yet, physiologically speaking, it is not necessary to give young children meat every day.*"

Some of the directions given as to the ventilation of school-rooms and other sanitary matters are sensible and judicious, and the denunciation of cruel modes of punishment is quite just, but we are curious to know what kind of punishment Dr. Budgett would permit in his ideal schools, as all we can gather is that he is opposed to *every* kind of punishment whatever. He does not tell us how any school has been practically conducted on such a principle, and we must suppose that all the scholars he has in view are good little boys and girls, who love learning for its own sake.



## Original Communications.

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### I.—Miliary Sclerosis—its Pathological Significance.

By W. B. KESTEVEN, F.R.C.S.

AN outline of the following remarks was embodied in a paper read before the Medical Microscopical Society, of London, on the 20th March. The object of that communication was to endeavour to draw the attention of histologists to a lesion which appears to be almost co-extensive with organic disease in the nervous centres, but which nevertheless has, until recently, been overlooked. This circumstance may excite surprise, when it is borne in mind how large a number of diligent and scientific pathologists have been for many years past engaged in the investigation of pathological histology.

The great diversity of diseased states in which miliary sclerosis is met with, in the brain and spinal cord, constitutes an important point in its pathological history, and possesses, in no less degree, an interest relatively to its clinical history, if indeed it can as yet be said to have any clinical history at all.

Miliary sclerosis was first described by Drs. Batty Tuke and Rutherford, in the pages of the 'Edinburgh Medical Journal,' 1868-69, and the lesion was so designated by them from the resemblance of the spots to millet-seeds in minuteness, compared with the more extensive forms of an analogous change which had been named by the French pathologists, general sclerosis, disseminated, annular sclerosis, *sclerose en plaques*, &c.

The name sclerosis is open, in some degree, to objection, inasmuch as hardness is not always its distinctive character. It would be more accurately expressed by the term "grey degeneration," of nerve tissue. Miliary sclerosis, or grey degeneration, is a change commencing in the neuroglia or its nuclei, the normal tissue having disappeared, is replaced by the minute millet-seed patches, either singly, or in clusters. These are very abundant in some instances. In one specimen that I examined from the brain of an idiot, they existed in the proportion of twenty-five thousand to a square inch of surface.

To attempt to describe in detail the characters of miliary sclerosis, would be merely to reproduce the exact and very precise description given by Drs. Tuke and Rutherford, as above mentioned; and still

more recently by Dr. Tuke again, in this Journal (July, 1873). In connection with this subject the reader may, with advantage, consult two papers on amyloid degeneration and brain sand, by Dr. Arlidge,<sup>1</sup> who has pointed out the tendency under morbid influence to the development in nerve tissue of albuminoid corpuscles, which in time undergo a calcareous transformation. The scope of the present remarks embraces mainly the *pathological relations* of the lesion under consideration. It is associated, as will be shown below, by widely different nosological conditions. It is associated also with other pathological changes, *e.g.* with amyloid and colloid bodies,—and differs essentially from what is known as the yellow granular degeneration, as well as from disintegration of nerve tissue, which may be either ante or post-mortem. The reactions of amyloid with codeine, in the brain and cord, are obscured by the presence of chromic acid employed in the previous hardening, but the spots of sclerosis may be distinguished from amyloid bodies by the use of the polariscope, which will give neither the concentric rings nor the black cross of amyloid bodies, while it renders evident the molecular character of the degeneration.

Colloid bodies may be distinguished by their clearly defined margins—miliary sclerosis presenting more or less irregular borders, from the broken ends of fibrils, vessels, &c., encroaching on their space—and from their clear homogeneous translucent contents, which do not take the carmine colouring. “In extreme cases the appearance of sections containing colloid bodies may best be compared to a slice of sago pudding, for they exist in such large numbers as almost completely to fill the field of the microscope, separated slightly from each other by a fine granular material.”<sup>2</sup> “Colloid bodies do not undergo the same gradations of development as miliary sclerosis, they do not push aside the fibres, and they never can be removed as a separate substance from the dried sections in which they exist.” These, Dr. Tuke points out, are the chief characteristic differences between colloid and miliary sclerosis. Having thus briefly alluded to the physical characters of miliary sclerosis, it becomes a point of great importance to determine, if possible, the pathological relations of these deposits,—their relation to previous diseases as observed during life. Here I think that at present we find ourselves at a loss. This lesion is found to co-exist with so many and varied morbid conditions, that we are puzzled in which direction to look for a clue to its proper place in neuro-pathology. In our present state of knowledge, we have no guide to the connection between this degeneration and the symptoms noted during life. In confirmation of this unsatisfactory view of the matter, I may state that over and above the examination of brains after death from insanity,

<sup>1</sup> ‘Brit. and For. Med.-Chir. Review,’ October, 1854, p. 470.

<sup>2</sup> Tuke ‘Brit. and For. Med.-Chir. Review,’ July, 1873, p. 208.



by Dr. Batty Tuke, already referred to, I have notes of my own observations of its presence in the brain or spinal cord, or both, in twenty different pathological conditions, as follow:—

1. *Acute meningitis* of the cord; following upon fracture of the skull, with hernia cerebri—death after two months. This specimen was given to me by Dr. Moxon.

2. *Abscess of the brain*.

3. *Locomotor ataxy*,—in the posterior columns, principally.

4. *Idiocy*, in two cases. One of these cases an example of microcephalic brain of a boy, aged twenty years, whom I had known from the time of his birth. The second case also was microcephalic, twenty-one years of age, accompanied with mollities ossium; idiotic from birth.

5. *Leukæmia*,—portion of spinal cord given to me by Dr. Moxon.

6. *Chorea*. (History wanting.)

7. *Tetanus*,—from several instances in which the course of symptoms had been rapidly fatal.

8. *Sub-acute myelitis*,—the spinal cord in this case was referred to Dr. Lockhart Clarke and myself, by the Pathological Society. (The Report thereon was presented to the Society in November last.)

9. *Pseudo-muscular hypertrophy*. The details of this case and its post-mortem examination were given by me in the 'Journal of Mental Science,' 1870. I should here observe that I at that time attributed to the presence of miliary sclerosis a more special pathological relationship to the case there related than a more extended observation has justified.

10. *Paralysis with Aphasia*. Case reported by Dr. Glover, in the 'Transactions of the Clinical Society,' 1873.

11. *Hydrorachis interna*; two cases,—in the first, a portion of spinal cord exhibiting this condition was given to me by Dr. Moxon for examination. This case is one of pathological interest with reference to the importance of following up any indication of disease, however unlikely it may seem to promise useful results. Dr. Moxon, examining the body of a boy who had died of a disease not specially involving the nervous centres, observed a band of old lymph deposit, of about a hand's breadth, encircling the inner surface of the costal pleura. This appearance led Dr. Moxon to examine the spinal cord, in the cervical and dorsal regions of which the central canal was found to have been dilated and distended with fluid. Both the white and grey matter of the spinal cord I found thickly studded with miliary sclerosis.

A second example of the same co-existence of miliary sclerosis and hydrorachis I found in a case of Progressive Muscular Atrophy, the particulars of which are reported by Dr. Headlam Greenhow in the fifth volume of the 'Transactions of the Clinical Society.'

12. *Apoplexy with hæmorrhagic softening*, in a portion of brain given me by Dr. Dowse, Superintendent of the Highgate Metropolitan Infirmary.

13. *Infantile convulsions*, in a child seven months of age, apparently healthy, but having a syphilitic history, suddenly attacked with fatal convulsions.

14. *Dementia*. A case of chronic mania that had been many years under my observation.

15. *General paresis*. Of the occurrence of this lesion in general paralysis, I have seen several examples.

16. *Malignant disease of the spinal cord*. For this pathological specimen I am also indebted to the kindness of Dr. Moxon. The preparation had been many years preserved in spirits of wine. It consisted of a small cauliflower growth, proceeding from the lining of the central canal.

17. *Glioma of the Pons varolii*. The opportunity of examining a portion of a tumour of this kind was afforded me by Dr. Hughling Jackson. Besides the special structure of the glioma, this tumour presented some examples of the colloid bodies.

18. *Internal hæmorrhage*, the cause of sudden death. A woman aged twenty-five, eight months pregnant, giving way to a violent outbreak of passion, died suddenly. The peritoneal cavity was found to be filled with blood, but the source of hæmorrhage could not be found, nor could any change in the vessels be discovered. Miliary sclerosis was found in the brain.

19. *Puerperal mania*. This case which presented several points of pathological interest, has been already related to the Pathological Society of London. Briefly stated, the principal facts are as follow:—The patient, who had been under my own notice, had borne seven children. After the last four births, she had on each occasion an attack of puerperal mania of a very violent character. In the last she sank from exhaustion. Permission being given, we removed the brain and spinal cord, in both of which abundance of spots of miliary sclerosis were found. I may add, that a very remarkable condition was found in the cord, viz., a total absence of the cells of the posterior vesicular column, for about an inch in extent on the left side of the lower dorsal region. No symptoms existed during life by which attention was drawn to the spinal cord. We are ignorant of the exact functions of this column of cells, but they doubtless have some relation to sensation.

20. In the pages of this Journal, April, 1869, I described “a new lesion in the medulla oblongata.” From the description then given, Dr. Tuke recognised “miliary sclerosis,” and was good enough to forward to me a copy of the paper, above referred to, by Dr. Rutherford and himself, in the ‘Edinburgh Medical Journal.’ From this I learned the real nature of my “new lesion.” I had no



history of the case whence it was taken ; the specimen had been given me by Mr. Bryant simply as material for microscopical study.

To this catalogue I could have added several more, if I had carefully preserved notes of all my microscopical examinations. The list, however, is sufficiently extensive to arouse a desire to know if, among all these various morbid conditions, any one common feature can be traced. To myself the search has hitherto met with small success. There is little in common between acute meningitis and locomotor ataxy, or between tetanus and local softening in the brain. In acute disease of the nervous centres, such as meningitis or tetanus, we must assume that the lesion pre-existed, since we cannot suppose such wide-spread degeneration to have been produced in the course of a few days. There is, moreover, no other lesion that occurs to my mind which, like the present, is met with equally in acute and in chronic disease of the nervous structure. That it belongs to some chronic morbid condition of the nerve tissue, appears to be the inevitable conclusion. The relation of this condition to symptoms remains yet to be discovered.

It has been suggested to me by Dr. Dickenson, whose opinion, upon all matters pathological, commands respectful attention, that this supposed lesion is but a physical post-mortem change, resulting from the action of alcohol on the tissues. But to this it may be replied that the same thing is found in brain and cord that have been preserved in chromic acid only : moreover, it is not always found in brain or cord that has been preserved in spirits of wine.

That in miliary sclerosis we have a really morbid degeneration of nerve tissue there can, I believe, be found no good grounds to doubt. The multitude of its associated pathological states, its wide diffusion throughout both brain and cord, may present difficulties in the way of solution of the question of its relationship ; but these circumstances afford no proof that it is not a veritable morbid change. What may be its starting-point is to be solved only by further investigations into the origin of the diseases with which it is associated.

So far as the present amount of evidence permits, this extends no further than to the inference that in a large number of diseases of the nervous centres, the significance of miliary sclerosis is a slow change or degeneration of the neuroglia, preceding or underlying the group of symptoms which have constituted the antecedent malady.

The accompanying illustrations present the characters of miliary sclerosis, as shown under low and high powers of the microscope, viz., from three and a half inch to one-eighth objectives.

## EXPLANATION OF PLATE,

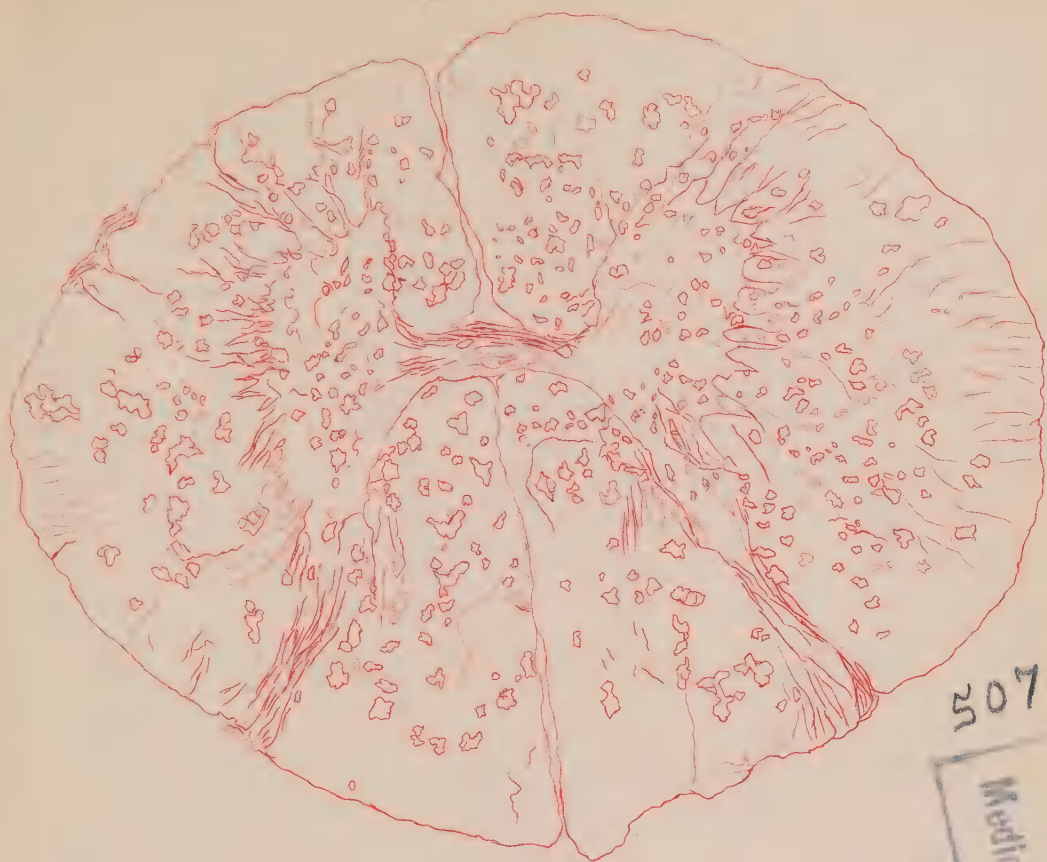
*Illustrating Mr. W. B. Kesteven's paper on Miliary Sclerosis.*

Fig. 1.—Section of spinal cord from a case of Tetanus, showing spots of miliary sclerosis. Seen under a three-inch object-glass.  $\times 12$  diameters.

Fig. 2.—Spots of miliary sclerosis seen under a quarter-inch object-glass.  $\times 195$  diameters.

Fig. 3.—The same under an eighth.  $\times 420$  diameters.

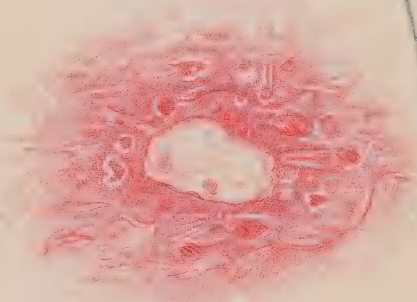
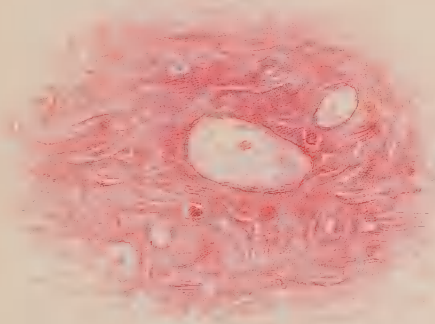




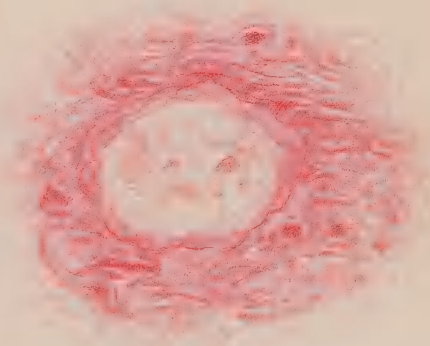
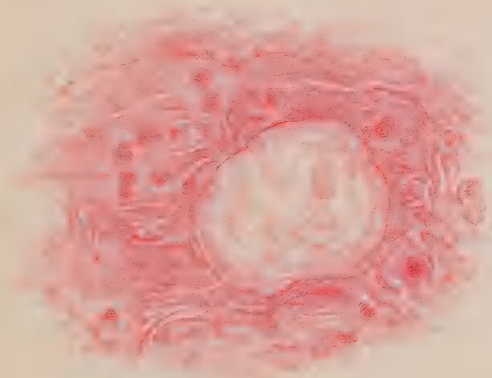
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## II.—Résumé of the Report on Epidemics of Cholera in France,<sup>1</sup> in 1832, 1849, 1855 and 1865.

THE Academy of Medicine having appointed a committee of its members to investigate cholera, a number of reports were submitted to that committee on the epidemics which at various times have visited the country. From them an able report was drawn up by M. Barth; but for our present purpose it will be sufficient to notice only some of the conclusions at which an endeavour has been made to arrive—for it must be confessed that few, if any, have actually been reached.

France has been visited by four great epidemics of cholera, besides some partial explosions, which seem to have been appendices to them. Thus, there were, (1) the epidemic of 1832, followed by a short re-appearance, in 1834, on the shores of the Mediterranean; (2) that of 1849; (3) that of 1854, which began in November, 1853, finished in January, but recurred in March following, and did not disappear till 1855; and (4) that of 1865, which broke out in June, continued throughout that year and the following, not disappearing till 1867.

With regard to direction, those of 1832, 1849, and 1854 invaded the north of France, advanced westward, ending in the department farthest in that direction. That of 1865, on the contrary, appeared on the French shores of the Mediterranean, extended northwards, and reached the remote parts of Brittany and Normandy. The epidemic of 1832 broke out in Paris with great violence in the latter days of March, ravaged several districts like a hurricane, attacked in succession 56 departments, ending the same year, after destroying 110,000 to 120,000 persons. That of 1849 also appeared in the end of March. It for a time was restricted to the parts immediately around Paris. It then attacked the city with a violence hardly less than that of 1832; ravaged 57 departments, and disappeared before the end of the year, having caused 100,000 to 110,000 deaths. The epidemic of 1854, on the contrary, began at the end of October, 1853, appeared to diminish soon afterwards, 'rekindled' in March, having Paris for its principal theatre. Thence it extended over 70 departments, and ended the following year, after having destroyed more than 140,000 victims. That of 1865 appeared in June, prevailed for some time at Marseilles and Toulon; some months afterwards it appeared in Paris, where it lighted up in the

<sup>1</sup> *Rapport sur les Epidémies du Cholera Morbus qui ont régnés en France pendant les années 1854 et 1855.* Par M. Barth, au nom d'un Commission, &c. &c. Paris: P. Masson, Editeur, 17, Place de l'École de Médecine, 1874.

following summer, causing at the same time cruel ravages in Amiens. It continued throughout the winter in the north-east of France, and was not extinguished till the end of 1867, having visited fewer departments and caused much less mortality than either of the preceding epidemics. Thus, while the epidemic of 1854 had a shorter period of duration, and caused more victims than either of the two preceding invasions, that of 1865, slower in the progress of its movement, continued longer, yet caused less mortality than either of the others.

In the two first epidemics the northern part of France principally suffered. Of 56 departments invaded in 1832, 52 only were so also in 1849. In 1854 the epidemic had a greater extent; it produced great mortality in Paris, and stretched thence to the Alps and the shores of the Mediterranean. Of 70 departments invaded by it, 54 had been previously ravaged by one or two epidemics, and of 16 respected in 1854, six had been previously visited by the disease. At the end of that year only 9 departments had remained unvisited during the several epidemics. Of these, one was slightly visited in 1855; the eight others preserved their immunity during the invasion of 1865-6. Thus, certain regions were visited by all the epidemics; others were visited by one or more, while a few have continued exempt from all. As a rule, the districts most spared were those in the centre of France, and most elevated above sea level; on the contrary, those most densely inhabited and of inconsiderable elevation, suffered most. In this, however, there is nothing either absolute or constant. Thus, in 1854 the epidemic was in sixteen departments more fatal than in that of the Seine. Amiens, previously spared, suffered greatly in 1866; Rouen was exempted in 1854, and Lyons was only slightly visited by any of the epidemics. For a time explanation of this relative immunity was believed to exist in conditions connected with the nature of the soil. The exemption of the regions in the north-east, in 1832 and 1839, was attributed to the existence of sandstone of the Vosges; yet that region was mostly dealt with in 1854. In a similar manner, parts of an arrondissement or canton spared at one time, have been ravaged at another; while others, ravaged by the early epidemics, were exempted by subsequent ones, although they continued to rage around them.

In some places the epidemics of 1854 and 1865 were more distinctly preceded by derangement of the intestines than those of 1849 and 1832. Such derangement was by one set of observers looked upon as premonitory of the epidemic; by others, as independent of it, and only caused by ordinary causes incidental to the heat of summer; by some, as due to fear of the epidemic. In many localities, however, the epidemic suddenly appeared while the state of the public health had till then remained satisfactory.



In all the epidemics the disease showed itself in the forms of cholerine and confirmed cholera. The affection becoming developed in a person arriving in a healthy district from one infected, seemed to have a period of incubation, varying in length from one to four days. Whether in the form of cholerine or cholera, the attack for the most part occurred at night; this circumstance being variously attributed to the process of digestion after a heavy meal, to the body being at rest, and to the non-renewal of fresh air around the person. Sometimes the attack was sudden and severe, death taking place in a few hours; more frequently it was slower, being preceded during some hours or days by diarrhœa,—an indication of great importance when it does occur, as treatment employed in this stage often checks the further development of the disease, and consequent mortality by it. Diarrhœa is so constant a symptom, that its absence, recorded by some authors in cases of what they called *dry cholera*, is denied by the majority, or looked upon as an error of diagnosis. For the most part it is the principal indication of the disease, and that which first appears; being followed in their turn by vomiting, cramps, cyanosis, failing of the pulse, coldness of the extremities, alteration of the voice, and suppression of urine. All these indications characterise the several epidemics, presenting only some slight modifications here and there, as less abundant dejections, less violence of cramps, less cyanosis, and so on. In the epidemic of 1854, the dejections contained more lumbrici than in any of the others, these entozoa besides being thus evacuated appearing sometimes in the matters vomited. The appearance of characteristic complications seemed less sudden in the epidemics of 1854 and 1865, in some places, than in those preceding; in others they were more so, and the progress of the disease to death in many more rapid. In some marshy districts the disease presented some characters of intermittence more or less marked. In some instances, in all epidemics, the disease did not advance beyond the state of cholerine, convalescence more or less rapid taking place from it. In other instances diarrhœa, nausea or vomiting, cramps, and the other characteristic symptoms, were present in inconsiderable severity (*cholera leger*); but more frequently there were repeated evacuations of serous fluid, succeeded by violent and painful cramps, great coldness, alteration of expression, voice sunken, and pulse imperceptible (*cholera grave*), but between these extremes there were intermediate cases, having neither the moderation of the first group nor the intensity of the second (*cholera moyen*). For the most part death took place in the algide stage; often, however, where treatment was early employed, reaction was obtained, characterised by diminution of the evacuations, cessation of vomiting, appeasement of cramps, re-establishment of the pulse, and return of warmth. This reaction, when excessive, gave place to congestions of the encephalon

and lungs, followed by prostration and stupor, designated a typhoid state. In these respects the epidemics of 1854 and 1866 presented similar characters to the two preceding, with merely slight modifications. In the two latter, papular and erythematous symptoms, diphtherite and swelling of the parotids, were more frequent than in the former, and further, a diminution in the numbers of attacks and deaths by ordinary diseases was observed during the two last epidemics. In 1854 an exception to this occurred in regard to the prevalence of *sudamina*, or sweating sickness. It continued on that occasion to affect one set of patients, while cholera attacked another, the two diseases appearing to run their course independent of each other. In some instances, however, they succeeded each other in the same individual; the occurrence of *suette* at times seeming as preparatory of cholera, in others, seeming to act as a preservative against it. In all the epidemics, relapses and the occurrence of sequelæ of the disease were observed. In some instances, persons who had been affected in one or both the epidemics of 1832 and 1849, escaped in 1854; others were attacked in each of the epidemics which occurred, and some who had recovered on the two earlier occasions being carried off in 1854.

The duration of the disease in the several epidemics was very various. As a rule, it was of shortest duration in the early period of an epidemic, the patients often dying in eight to ten hours. In the most part, however, they succumbed in thirty-six hours to two days in the algide stage, or at a later period their reaction and the typhoid state set in. Excepting slight cases, recovery did not take place completely in less time than a few days. In the greater number of instances, especially if attended by reaction, it did not take place in less than two weeks; often convalescence was slow, being accompanied by persistent debility. Frequently the occurrence of cholera causes the death of the fœtus in uteri, and induces abortion. In a few cases abdominal dropsy has disappeared after an attack of the disease.

In all the epidemics, it was observed that cadaveric rigidity set in early. In some exceptional cases certain movements of contraction and extension in the limbs occurred after death. The anatomical changes did not vary during the various epidemics. The isolated follicles of the intestine were exaggerated; the heart and vessels contained ropy black blood; there was more or less injection of the mucous membrane of the digestive tube, suffusion of the lungs; vascularity of the pia mater and mucous centres in cases of death after reaction. These are the appearances found in all seasons, and in all latitudes.

On the subject of etiology, it is observed that cholera has visited regions the most diverse,—continents and islands,—all latitudes,



from the equator to the polar circle,—climates the most diverse, and elevations above snow level. With regard to France, it has prevailed in all seasons, temperatures, and barometric conditions,—in dry weather and in wet,—under electric conditions the most diverse, and in all conditions of the wind. From the reports received, however, it is clear that a high temperature is favourable to the development and increase of the disease, it being during the hot season that the attacks are most numerous, and in the cold that they are less so. According to the reports of 1854, great and violent atmospheric perturbations, as hurricanes and sudden variations of temperature, are often followed by an increase in the number of attacks and deaths by cholera. The wind seems to exert an influence as an agent of propagation of cholera at a short distance, and places fully exposed have been found more severely affected by the epidemics than others that were more sheltered. In France the disease prevailed throughout the four arrondissements, at altitudes the most diverse, including the mountains of the Vosges, Jura, Alps, and Pyrenees, and on the plains that border the Mediterranean. It has prevailed in districts of different geological formations, including primitive, secondary, and recent; on the sides of hills, and in valleys, plateaux, and plains; bare and wooded districts; waste and cultivated; upon dry soils and upon humid; sandy, gravelly, and clayey; chalky and marshy; quartz, sandstone, and granite; in districts the most healthy, and in those that are unhealthy. As a general rule, however, it is observed that low situated localities, and those that are marshy, are especially favourable to the development and spread the disease, and that such as are elevated and naturally healthy, afford comparatively the greatest security.

As no kind of locality remained free from the disease, so no class of persons were exempt. All social grades and positions suffered from it. In some places, even where the hygienic and social conditions seemed most favourable, the rich and well-to-do suffered the most severely; nevertheless, the result of careful analysis has been to show that cholera has prevailed most where the hygienic conditions have been bad;—that want of air, crowding, insufficient clothing or warmth, fatigue, bad food and privation, all conduce to it; also, that depressing moral influences have the same effect. In addition to these, the development of the epidemic is favoured by the occurrence of inundations during the hot weather, contamination of the water, vitiation of the air by telluric and foetid emanations of various kinds, great agglomerations of people, and in fact, all violations of hygienic laws. Several reports specially indicate the liability to attack of such as are employed in attendance upon the infected, as also of those who have to deal with and wash the linen used by them, and with the interment of the dead. No age was exempt from attack; yet the two extremes of age and youth

furnished the largest proportion of deaths. The weak and enfeebled offered least resistance to attack ; yet the strong and robust were in no way exempt from it. Those suffering from chronic diarrhœa, from phthisis, and such as were convalescent, seemed also to be peculiarly exposed to it. There appeared moreover to exist an individual predisposition to attack ; and it is remarked, in regard to the epidemic of 1854, that this predisposition was traced in numbers by consanguinity of the same family, rather than among such as were only related by marriage.

Various opinions have been expressed in regard to the nature of the disease. It has been considered to consist of a species of intermittent fever, an affection of the ganglionic system, a variety of typhus, a disease of the liver, a special fermentation, a new disease *sui generis*, the cause being according to some unknown, according to others, existing in conditions which induce sporadic cases of cholera, and these becoming more general and intense affect the masses in the form of an epidemic. Others look upon it as consisting of a miasmatic poisoning, of an unknown nature, affecting the nervous system, of organic life, or the blood, and entering the system by the pulmonary or digestive canals. This uncertainty in regard to the nature of cholera is reflected in the different opinions expressed as to its propagation. According to some authors, the disease springs up, is developed and multiplied, in a given locality under the influence of various morbid causes, becoming fatal by their accidental co-existence. Others add to the presence of insanitary conditions, the concurrence of accidental causes, such as modification of the air by the existence of diseases of the vegetable kingdom, and general debility arising from deterioration of alimentary substances. Some consider that it is imported by persons arriving from an infected locality, that the disease becomes multiplied and developed, and communicable from one to another by *mediate* contagion, in the same way that typhus does. This theory, generally denied in 1832, was more frequently brought forward in 1847, and in 1854 became predominant,—sixty-one out of ninety-seven authors who allude to it expressing their opinion that cholera is thus transmitted.

In 1865 the opinions in favour of the importation and mediate contagion of cholera became still more general. It had appeared at Mecca immediately after the arrival at that place of a caravan of pilgrims from India ; it increased in an atmosphere rendered pestiferous by the presence of thousands of putrefying dead bodies ; it broke out at Cairo and Alexandria with the return to those places of the pilgrims ; it invaded afterwards Constantinople, then Mocha, Barcelona, and Marseilles,—these being the ports between which and Alexandria communication was most frequent ; and while it extended in other directions, Sicily, at which no vessel touched, escaped. At the same time, attention being awakened, it was easy to follow the



track of successive explosions, and to trace with certainty the transmission of the disease from one individual to another.

Methods of treatment the most various were employed. Bleeding used in 1832, was less frequently had recourse to in 1849. In 1854 it was used against complications, as secondary fever; but in 1865 was too much forgotten from the tendency there is to pass from one extreme to another. Opium, vaunted by one set of authors, was condemned by another, so also with ipecacuanha. Evacuants, praised by some, were declared by others to be deadly. Antispasmodics, notably ether and chloroform, were generally employed with advantage against spasmodic phenomenon. Frictions and excitants to the surface were used, but opinions in regard to them seem to be unrecorded, and a similar remark holds good in regard to the employment of cold. Quinine, generally without success in the algide stage, was a little more efficacious during secondary fever. Sulphate of strychnine and nux vomica were either followed by want of success or by distinctly unfavorable results, and the same is said of valerianate of zinc. Sulphate of copper, reported on favorably in one place, was condemned in others. Without enumerating many other remedies, homœopathy was employed in many places. At Dienville, of sixteen cases thus treated fourteen died; and at Marseilles, in 1854, out of twenty-six similarly treated, twenty-one died. It is added that where success followed remedies it was mainly due to the hygienic care, particularly to the means taken to keep up the temperature of the body. Some writers disbelieve in the power of medicine during the algide stage, observing that the function of absorption is then destroyed. Indeed, whatever methods were adopted in the epidemics of 1854 and 1865 the results were equally unfortunate as in the two preceding, namely, 50 per cent. of the patients died. When the rate of mortality is only one-third of those attacked, the results must be considered satisfactory, and when under that, as exceptional; in fact, statistics showing a smaller death-rate are open to mistrust. The manner of preparing statistics varies also; some writers include all cases, whether severe or slight, even simple diarrhœa, thus showing very favorable results, even when actual success is the reverse. Different degrees of strength in patients themselves also influence the rates of mortality. As a rule, an epidemic is more severe at its commencement, and less so in its decline; more fatal in infancy and old age; less so in youth and mid age; more limited in civil life, where patients are isolated; more fatal in hospitals, where it attacks organisms already debilitated, and where cases of the most severe nature are taken.

According to the majority of writers, the principal cause of mortality is the too frequent neglect of the early symptoms, and delay in employing treatment. Nearly all insist upon the necessity to combat the disease at its origin. A great many writers place

*preventive* treatment, even before the curative, and only see safety in the application of hygiene to preserve persons and localities threatened by the disease. These measures consist, for the individual, of temperance, and care to avoid fatigue and excess of all kinds; for the inhabitants, cleanliness, renewal of air and ventilation, disinfection of threatened houses, and especially of such as have been attacked; the removal of dejections, the disinfection of matters and linen soiled by excretions, and the prompt interment of the dead. The wards of hospitals ought to be fumigated, and chlorides sprinkled in public places. Some reports suggest the disinfection of the atmosphere, either by fires of oil or by means of blasting-powder, and in confirmation state that in the village of Thienant the epidemic suddenly disappeared after the occurrence of a fire, which consumed several houses. House-to-house visits, for the purpose of early discovering persons attacked is recommended. The committee observes, with regard to these measures, that although all are very proper, yet, would it not be better to strangle the evil at its primary source, or, at any rate, to prevent it from reaching the country?

Here arises the question, What is cholera? Whence comes it? What is its origin? How is it developed? Cholera, such as in former times prevailed in France, is a new disease in Europe, and *sui generis*; nor does the history of epidemics afford an exact description of the disease as it has been seen there. Notwithstanding the identity of the name with epidemic cholera, the sporadic is essentially different from that disease, both in its nature and in its great severity. The one is deadly, the other benign; the relative severity of the two being compared to the sting of the Indian scorpion and that of Europe. The sporadic form—the product of causes easily appreciable, as for example, cold drinks during the hot season—is altogether of an individual character, and by no person believed capable of being communicated by the subject of attack to another. The epidemic form is often developed without the presence of sufficient appreciable cause, affecting a large number of persons and places, under hygienic conditions the most diverse, and often transmitted from one individual to many others.

1. It is universally admitted that India is the home of the disease, especially the districts near the large rivers, as plague prevails in the delta of the Nile, and yellow fever at the mouths of the Mississippi. According to one set of pathologists, cholera is the product of a special miasma originating in India, transported into Europe across continents and seas by atmospheric currents; according to others, cholera is produced on the spot where it shows itself, engendered by accidental local conditions, such as are called epidemicity, and multiplying by the action of special causes on the mass of the population; a third class consider that cholera, originating in India under particular conditions of climate, propagates itself to



long distances, transported by man, and multiplying by successive transmission, favoured by accidental causes. In opposition to the first theory various objections of great weight are adduced. The exhalations from the Pontine marshes do not produce fevers at a great distance from their source. Let us therefore admit for cholera effluvia a power even a hundred times greater, it would be insufficient to account for its transport to a distance of four or five thousand leagues from its point of origin. Is it admitted that the miasmas become multiplied in the atmosphere? How are we to understand that effluvia, arising in India in 1817, and carried by the atmosphere, required fifteen years to traverse the 2500 leagues which separate the banks of the Ganges and those of the Seine? What slowness, when hurricanes traverse the breadth of France in one day! How does it happen that these miasmas propagate themselves in different directions?—that from the Bay of Bengal they extend eastward to Sumatra, Borneo, and China; southward to Ceylon, Malabar, and Mauritius; northward to Tartary; north-west to Persia and Egypt, and thence across Russia and Europe? How are we to explain the circumstance that, in 1832, cholera passed from Germany to London without affecting Belgium; passing also from London to Paris while strong north-east winds prevailed, and with a clear sky? Is it said in reply, that in the higher strata of the atmosphere there are currents which proceed in an inverse direction to those that are apparent? The supposition is gratuitous. And according to the committee, the epidemic was carried by travellers fleeing from their foggy country already infected, to enjoy the advantages of Paris until that time free from the disease. How also are we to explain that never, in any country, has the extension of cholera coincided in a definite manner with the direction of the prevailing winds? How are we, according to the theory of transport by the atmosphere, to account for the singular tendency of the disease towards certain great cities in different epidemics? Thus, in 1832 Calais was first invaded, then Paris. In 1849, Douai, and immediately afterwards Paris. In 1853, the departments of Aisne and Paris within a few days. In 1865, Marseilles and Toulon, then Paris—always Paris. What is the situation of Paris in regard to all currents of wind from east, north, north-east, and south? Is it not rather that there is always a stream of the population itself towards the great social centre of France? It has been stated that cholera has often appeared to extend along the course of streams and rivers, and it has been supposed that this arises from the contamination of the air from the beds of those streams and rivers. But experience teaches that cholera as often ascends along the course of a river as descends by it. Moreover, is this to be explained by the circumstance that habitations are generally most thickly placed along the vicinity of rivers? If the epidemic were conveyed by the atmo-

sphere, it might be supposed that the influence would descend suddenly upon a number of places in the interior of a country like a shower of hail. Instead of doing so, it invariably enters from the borders, most frequently from a sea-port. How again, supposing that it was conducted in the form of a miasmatic cloud, is it to be supposed that that cloud would remain during three months extended over a city, as for example in the case of Amiens? How, on the same hypothesis, are we to account for the epidemic being limited to one district, one street, or even one range of buildings?

2. Against the theory according to which cholera is generated upon the spot where it appears, various objections are adduced. The precise causes and conditions under which under such circumstances the disease is produced, are left undefined. Are its causes looked for in special conditions of the atmosphere of the locality? The conditions showing the prevalence of an epidemic are precisely similar to what they are in its absence. Is it high temperature? In 1846 and 1863 the heat was  $36^{\circ}$  and  $39^{\circ}$  Cent. ( $97^{\circ}$  Fahr. and  $102^{\circ}$  Fahr.); yet no case of cholera occurred. If a high temperature favours the development of cholera, it cannot be looked upon as its cause. In the last epidemic at Cherbourg, the greatest intensity of the disease occurred in the months of January and February, 1866; and during 1830-31 it prevailed at Moscow in the winter season. Is humidity or dryness the cause? Dryness was extreme in 1846, humidity in 1852, yet cholera did not prevail in either year. Is it variations in the state of atmospheric electricity? If so, what are they different from such as take place many times without the appearance of cholera? Nothing positive on this point has been ascertained. Are the causes to be found in a changed condition of the constituent principles of the air, a diminution of ozone? Decrease of ozone occurs every summer, and the supposed changes in the constituent principles of the air have never been detected by analysis. Do the causes exist in the constitution of the soil? If so, the soil of France has undergone no change during the last thirty-five years—has been subject to no cataclysm. Have we not also seen cholera prevail in regions having the most various geological constitution?—low, damp or marshy alluvial localities near marshes, or infected streams favour the development of cholera, but are not sufficient always to produce it. Thus in some places, localities seemingly the most unhealthy have been respected, while others, to all appearance most healthy, have been ravaged by it; in the same department, arrondissement, or canton, certain villages ravaged in 1854 had escaped in 1849, and nine which had been free in 1849 and 1832, suffered severely in the later epidemics. Such conditions are very secondary. They may, and do have an influence upon the multiplication of cases of the disease, but none of these conditions, nor the union of several, have ever given rise to the disease. Do we



invoke a combination of hygienic conditions, such as want of sufficient space, clothing, air, light, or food, bad water, excesses of all kinds, mental depression, overcrowding, and so on? All these favour the development and multiplication of the disease, but no one or combination of them can produce the disease. To go no further back than 1792, have there existed no bad conditions since then? Had we not at the beginning of this century, throughout Europe, grand movements of armies, engagements, and battles, notably in 1813 and 1814; great military disasters, precipitate retreats, with their fatigues and privations, endured by coalesced Europe, great dryness, great humidity, scarcity, famine, typhus, and other evils?—yet in the midst of all these no case of cholera occurred.

3. As to the theory that cholera, originating in India, was imported and propagated by man, its partisans present the disease as following the great lines of communication by land and by sea. In the first great invasion, for example, they trace it from India, where it is said to have sprung up in 1817, advancing across the continent of Asia by the route of caravans and armies, traversing in succession Prussia, Russia, the north of Germany, and reaching France in 1832. In the latest invasion, on the contrary, it was observed quitting India in the early months of 1865, advancing towards Europe by sea, and reaching Marseilles in June, that is, less than six months from the time of its starting. On the first occasion it took fifteen years to reach France. In the latter it advanced with the rapidity of vessels, requiring only a few months to arrive from Calcutta and Bombay at Mecca, then at Cairo, Alexandria, the shores of the Bosphorus, Catalonia, and Provence. There exists no example, say the advocates of this theory, of the arrival of cholera from one continent to another more rapidly than the progress of travellers, and from one continent across sea more rapid than the rate of steam-vessels.

When the invasion takes place by land, it is indifferent by what point of the frontier the cholera penetrates. When it arrives from beyond sea, as from Alexandria in 1865, it first arrives in the commercial ports, as happened then in respect to Constantinople, Ancona, Barcelona, and Marseilles, all of which were affected before other places nearer the point of departure. The explosion of the disease also took place on all occasions soon after the arrival of a vessel from the infected country. The partisans of importation, moreover, argue, in regard to the preservation of certain islands and certain establishments, both public and private, which had for a time interrupted all communication with reported localities, or prevented the approach of all vessels arriving from suspected places. Thus, Sicily was preserved in 1865 by receiving no vessel arriving from affected localities; so also Batna, in Algeria, by means of posts of

surveillance, which isolated it in the middle of the country ravaged by the epidemic.

In support of the transmissibility of cholera from one person to another, the advocates of the theory adduce many instances where the arrival of one or several persons affected with cholera in a district, until then free from the disease, was immediately followed by a development of the disease among those who had received or attended them; on the other hand, the opponents of that theory only see in these events the occurrence of simple coincidences, or explain them by the intervention of epidemicity, which really means *nothing*. Others adduce numerous opposing facts against the theory, giving, as proofs of the non-contagion of cholera, the cases of many persons that had attended patients, slept in their beds, and yet did not take the disease. To this the partisans of transmissibility reply that in the instances alluded to negative facts cannot destroy the value of those of a positive kind, as where the transmission of the disease had the character of evidence. At the same time the non-contagionists, while denying the facts brought forward by their adversaries, claim for those adduced by themselves in refutation, the character of being also positive. The partisans of the theory of transmissibility, however, do not assert that cholera is always thus communicated. They admit the necessity of special conditions, without which the transmission does not take place, any more than does smallpox, scarlatina, diphtheria, and so on, conditions which, as in the case of typhoid fever, depend partly upon the intensity of the morbid principle, its condensation in a limited locality, the duration of its action, &c., partly upon the powers of physical and moral resistance possessed by the individuals exposed to contagion, and their degree of susceptibility.

The partisans of epidemicity deny, as a gratuitous supposition, this condition of special aptitude deemed by their adversaries so essential for the transmission of the disease; but are they not themselves obliged to admit the existence of a partial predisposition in those attacked by the disease, and also an individual peculiarity in those who resist? How otherwise are we to account for the epidemic influence hovering over a locality only attacking some individuals, and not all? The contagionists believe that the hundreds of instances in which communication has not been traced do not invalidate a single fact of true transmissibility; thus, it is observed in the report, the cases of three medical men, whom they name, as having contracted fatal attacks of diphtheria from patients, prove the communicability of the disease, although many others who attend patients suffering from it altogether escape. One of the arguments long used against the communicability of cholera is the large number of medical men who in the first epidemic pronounced against it. This has, however, been abandoned in subsequent epi-



demics, and shown to have no value. Even in 1832 there were some who believed in contagion of the disease; they became more numerous in 1849, became predominant in number in 1854, and after 1865 there remained only a few opponents in the field. To refute the communicability of cholera, an argument is drawn from the small number of "sisters," students, and infirmiers, who became attacked with the disease. It may be said, in reply, that if the number of medical men and sisters is less considerable, this is to be explained by the moral power, and consequent resistance of these, to their sentiment of duty, and to their habit of struggling with disease; others refer this relative exemption to the solidity of their faith and their Christian resignation. It is asserted, on the other hand, that this pretended exemption is only an error. In 1849, at the Salpêtrière, the director, two internes, several attendants and infirmiers, succumbed, while a number of the medical men and employés were severely attacked. In 1865 eleven medical men died of the disease at Ancona, three at Paris in 1866, three medical men and thirty sisters at Amiens in the same year.

It still remains to explain the occurrence of many cases where persons have been struck at the same time and at places distant from each other, without having had any communication with other subjects of the disease, when there could have been no transmission by contact. According to the partisans of transmissibility, if cholera requires immediate contact with a patient suffering from it to be thus caused, it is communicable to a greater or less distance, according to circumstances, by the emanations from patients.

Notwithstanding the four epidemics of the disease that have ravaged Europe, a number of persons still hesitate to express their opinion on the origin and prevention of the disease. The theory of propagation of the disease by atmospheric currents suggests nothing against its course, to avert or destroy it. That of spontaneous generation of cholera, in like manner, proposes nothing, nor does it indicate where is the source of the disease, what is the cause of its production, or under what combination of conditions it is produced. On the contrary, the theory of importation of the germ of cholera, of its generation in persons successively attacked, and of its transmissibility from one organism to another, not only points to the most rational therapeutic indications, but also indicates the hygienic means best calculated to diminish the severity of the disease, and to formulate the international measures by which new invasions may be prevented. The committee, having weighed all the evidence adduced by reporters, and judging from the experience of its members, adopt the opinion of transmissibility of cholera as the most natural. They observe the frequent occurrence of the disease in the proximity of places already attacked, and the rapid development of the disease in

its subjects. They look upon the enormous dejections, often amounting to eight and ten litres in the space of a few hours, as only to be accounted for as arising from the serosity of the blood exuding through the surface of the intestines, but the albumen of which does not reveal itself by the ordinary reagents. They consider that the action of *ferments*, which attack by predilection the albuminous matters, is to transform and multiply themselves; that their corpuscles, soluble or insoluble in water, become suspended in the air, and are transported by that vehicle; that cholera originating in India is the product of a special miasm that has never been, and never can be, produced in France. This miasm, they consider, consists of subtle, impalpable corpuscles, probably organic in nature, but which have not yet been isolated by means of science. They penetrate into the living economy by the pulmonary and digestive organs, act as ferments, and produce in the albumen of the blood a modification of composition, the result of which is that the serum transudes by the surface of the intestine, thus causing decrease in the mass of the blood, the course of which in the capillaries becomes more and more slow. This morbid miasm becomes multiplied in the organism like molecules of ferments, is disengaged from the affected body chiefly by the alvine dejections, is suspended in the air, propagates itself to distances to affect new victims under particular conditions of temperature, which affect the powers and organic predisposition, or otherwise favour its effects. The primary source of the disease-producing agent resides in the patients, notably in the matters evacuated by them, not only during life, but after death. The emanations arising from the dejections become a direct and manifest cause of cholera in the vicinity of a patient, the more so when ventilation and cleanliness are neglected.

A second source resides in the dejections thrown upon refuse heaps in streets or courts, as in villages, farms, and badly-kept houses, deposited in public or private latrines, communicating by means of pipes in different storeys. The emanations thus arising from different points mixed with the air, and transported with it, become the means of propagation of the disease in different directions, according to the displacement of contaminated air and the intensity of morbid effluvia.

A third source, less apparent, proceeds from water contaminated by morbid dejections thrown upon public ways or into drains, and disengaged by subsequent rains, producing streams or percolating into the earth. Thus also cholera is induced by water from some wells, and that from cemeteries passing into sources of supply for towns and villages.

A fourth source of the disease exists in body linen, bedding, clothing, and other articles impregnated with the dejections of patients. These, being sent to a greater or smaller distance for



washing, become the means of transplanting the morbid principle into places until then exempt, the dissemination taking place with more or less rapidity, according to the rapidity of transport.

Lastly, patients being moved, whether individually or in masses, as emigrants, armies, or caravans, bring with them cholera into previously healthy places, even to considerable distances, and also more or less rapidly, according to the rapidity of their movements and facility of transport.

In these various ways cholera may be caused and propagated. The conditions necessary for both may exist in the chamber of the patient or in the ward of an hospital, in the vicinity of a focus formed by a *depôt* of alvine matters, whether removed to a distance from the affected or in their neighbourhood, or of those fleeing from infected localities, while the germs are as yet undeveloped in them, or have only produced cholérine. The circumstance that the disease on some occasions continues for a long period, affecting a large number of persons, while in others it is speedily extinguished, after making a few victims, depends upon the organic aptitude of individuals, such as we in fact have in consanguinity. It also depends upon the hygienic state of the inhabitants in regard to competency, temperance, cleanliness, distance from each other, or crowding of habitations, camps, or ships; according to the state of the locality where the germ is deposited—as to whether it is low, marshy, surrounded by hills, preventing free circulation of air, and so on. Thus we have an explanation of the disease being communicated to persons residing near patients suffering from it, of its extension in the same building, in a particular range of houses to leeward of a contaminated drain, whether in a village or town. A similar explanation may be given of the sudden occurrence of the disease at great distances without intermediaries, also in places considerably separated from those where it has first broken out, contact being thus traced in many outbreaks in appearance spontaneous. In this way we can explain the good results of removal of a body of troops from a contaminated locality, or a camp the ground of which is contaminated by putrid matters or dejections of cholera patients, and establishing them in a new and more elevated locality, the soil of which is still untainted.

With regard to treatment of those attacked by the disease, the best method is to neutralise the miasm, primary source of the malady, but the nature of that principle being unknown, the antidote has yet to be found. If science does not yet possess a special agent, it at least leads by way of induction to the employment of other agents, the utility of which has been demonstrated by experience. The principal indications to fulfil are, to moderate the enormous evacuations of fluid which take place; to maintain warmth of the surface to favour the circulation of the blood. The hygienic

measures to be adopted include the greatest cleanliness around the patients; free ventilation; the disinfection of clothing and dejections, and the speedy removal of the latter. Long continuance in the rooms or wards of the sick is to be avoided; the attendants should be often changed, and all superfluous persons sent away. As individual preventive measures, temperance and moderation in all things are to be observed; the state of the bowels must be attended to, and tendency to diarrhœa checked. Infected localities are to be abandoned when possible, and at any rate the departure of the timid will diminish the number of the predisposed, and thus lessen that of the victims. In respect to public hygiene, both before and after the outbreak of the disease, the public and private latrines, the drains and sewers should be disinfected; all causes of putrid emanations should be removed. Special wards for cholera patients should be established in hospitals, they being provided with free ventilation and ready means to change the bedding and linen as required. These wards should be maintained in the highest degree of cleanliness. If the epidemic be protracted, they should be changed from time to time, completely fumigated and whitewashed. The air of dead-houses should be purified as far as possible; the bodies of the dead should be speedily interred and surrounded by lime, or, as recommended in France, subjected to cremation. Water for use is only to be drawn from wells secured from pernicious underground infiltration, or from rivers above the point of discharge of drains and sewers into them. The quality of articles of food should be tested; tainted meat and unripe fruit should be condemned; and the sale of drinks supervised. House to house visits should be instituted, patients in the early stage of attack searched for, and treated or removed as the case may be. The dwellings should be cleansed. Paupers and tramps arriving from contaminated localities should be isolated in places set apart for the purpose, there receiving all needful care. Assemblages of troops, fairs, and markets are to be prohibited; troops should not march through infected places; they should be broken up and spread if already attacked, encamped in a healthy spot, and placed under shelter.

With regard to these measures, the committee observes that they are all excellent in their several ways; yet that more remains still to be done: other measures must be taken with a view to prevent new invasions of the disease. Of these, the best would unquestionably be, to check the development of the disease at its original source, and adopt the same sanitary measures as made the plague disappear from the delta of the Nile. If that be impossible the arrival of the disease must be prevented by measures, the efficacy of which has been proved by experience. Quarantine should be applied to caravans leaving India towards the north-east of Europe, also to ships from India or other infected places. It is known that



in 1865 the Mussulman pilgrims embarked at Jeddah carried cholera with them ; they disembarked at Suez, proceeded thence to Alexandria by rail, and it was only after a new embarkation that they reached Beyrout, Smyrna, and Constantinople. With a view to prevent a similar occurrence for the future, it would be well to prevent the arrival by ships of pilgrims from Arabia into Europe.

## Chronicle of Medical Science.

### REPORT ON SCANDINAVIAN MEDICINE.<sup>1</sup>

By J. W. MOORE, M.D., M.Ch. Dubl., F.K.Q.C.P.,

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1. A novel feature has been introduced into the volume of the 'Northern Medical Archives' for 1873, namely, a series of *Comptes Rendus* of the papers contained in it. Through the medium of the French language, an authoritative *précis* of the most valuable Scandi-

<sup>1</sup> *List of Journals, Papers, and Works, analysed in the above report :*

1. 'Nordiskt Medicinskt Arkiv.' Redigeradt af Dr. Axel Key, Prof. i Patolog. Anat. i Stockholm. Femte Bandet. Med 10 Taflor och 10 Träsnitt. 1873. 'Northern Archives of Medicine.' Edited by Dr. AXEL KEY. Fifth Volume. With 10 Plates and 10 Woodcuts. Stockholm. 8vo.

2. 'Hygiea: Medicinsk och Farmaceutisk Månadsskrift.' Trettiofemte Bandet. No. 1—12. Januari—December, 1873. Redigerad af Dr. A. JÄDERHOLM. 'Hygiea: A Monthly Journal of Medicine and Pharmacy.' Thirty-fifth Volume. Nos. 1—12. January to December (inclusive), 1873. Edited by Dr. A. JÄDERHOLM. Stockholm. 8vo, pp. 742.

3. 'Norsk Magazin for Lægevidenskaben.' Udgivet af det Medicinske Selskab i Christiania. Tredie Række, tredie Bind. Redigeret af JACOB HEIBERG. 'Norwegian Magazine of Medical Science.' Published by the Medical Society of Christiania. Third Series. Vol. III. Edited by JACOB HEIBERG. Christiania. 8vo, pp. 736.

4. 'Ugeskrift for Læger.' Tredje Række. Bind XV, XVI. Redigeret af Dr. F. TRIER. Kjöbenhavn. 'Weekly Medical Journal.' Third Series. Vols. XV, XVI. Edited by Dr. E. TRIER. Copenhagen, 1873. 8vo.

5. 'Upsala Läkareförenings Förhandlingar.' Åttonde Bandet. Häft. 1, 2, 3, och 4. Upsala. 'Proceedings of the Upsala Medical Association. Vol. VIII. Parts 1—4. Upsala, 1872 and 1873.

6. 'Hospitals-Tidende: Optegnelser af praktisk Lægekunst fra Ind- og Udlandet. 1873. 16 Aargang. Kjöbenhavn. 'Hospital Gazette: Notes on Practical Medical Science, Home and Foreign.' 16th Annual Volume. Copenhagen. Folio, pp. 208.

7. 'Forhandlinger i det Norske Medicinske Selskab i 1873.' 'Proceedings of the Norwegian Medical Society in 1873.' Christiania, 1873. 8vo.

8. 'Förhandlingar vid Svenska Läkare-Sällskapets Sammen-Komster, år 1873.' Protokollsförande: Med.-Rad. EDHOLM och Doktor WISING. 'Proceedings of the Meetings of the Swedish Society of Physicians in 1873.' Edited by Drs. EDHOLM and WISING, Secretaries. Stockholm, 1873. pp. 344.

9. 'Bidrag til Kundskab om de Kroniske Rygmærvsbetændelse.' Af C. LANGE. Kjöbenhavn, 1874. 'A Contribution to the Knowledge of Chronic Inflammation of the Spinal Marrow.' By C. LANGE. Copenhagen, 1874. 8vo, pp. 176.



navian contributions to medical literature will thus be placed within the reach of many to whom Swedish and Danish are unknown tongues.

This, the fifth volume, opens with a communication by Professor Drachmann, of Copenhagen, on "Arthritis Deformans," or "Chronic Rheumatic Arthritis." The author considers the disease chiefly in its clinical and pathogenetic aspects. Five important questions require to be solved; they are—(1) What is arthritis deformans? (2) Its symptoms? (3) Its relations to gout and rheumatism? (4) Its etiology? (5) The means of treating it? He adopts Haygarth's definition:—"The disease is almost exclusively peculiar to the female sex; shows itself without fever; the accompanying swelling of the joints is always attended with swelling of the bones; the integuments are not inflamed, and the muscles appear not to suffer."

The author's experience is based upon observations of 28 cases of the affection, *all of which occurred in females*. As regards the question of *age*, 4 of his patients were first attacked before they were 20 years old, 6 between the age of 20 and 25, 9 between 25 and 40, 5 between 40 and 50, and 4 later. One third of the cases were in easy circumstances, including several in the highest ranks of life. The malady most frequently began in the small joints, especially of the hands. Thus, in 18 instances the point of origin was the finger, the hip in 3, the elbow in 1, the knee in 2, the foot in 1; several joints were simultaneously attacked in 3. With regard to the *chronicity* of the disease, Professor Drachmann found a clinical history of under 5 years in duration in 6 instances, of between 5 and 10 years in 8, of from 10 to 20 years in 6, of from 20 to 30 years in 7, and of more than 30 years in 1 instance.

He tabulates the predisposing or exciting causes of the disease as follows:—Long continued over-exertion in 12 cases, cold and draughts in 9, damp dwellings in 8, a sedentary life in conjunction with cold in 3, menstrual disturbance in 3, rheumatic fever in 2, the grand climacteric in 5. He remarks that occasionally the attacked joints, instead of undergoing ankylosis, gradually become so relaxed as to simulate the condition termed by the Germans *Schlottergelenke*. One remarkable example of this is given. The Countess H—, aged forty-three, had suffered from *arthritis deformans* since she was eighteen. *All the affected joints were so freely movable as to readily undergo subluxation*. Böcher's observation as to the diminution of phosphoric acid in the urine, while it was quadrupled in the blood, of a victim to chronic rheumatic arthritis, led Drachmann to analyse the urine in 20 of his patients, with the result of confirming Böcher's statement. The mean quantity of phosphoric acid excreted per diem in the urine of these 20 patients was 1.194 grammes. Neubauer gives the mean quantity for an adult male as 3.5 grammes, and Dr. G. Harley for a healthy adult as about 3.22 grammes (50 grains).

Dr. Drachmann, in analysing the concretions so characteristic of chronic rheumatic arthritis, found their chemical composition to be identical with that of bone, excepting a slight preponderance of lime. He lays stress on this as a point of differential diagnosis from gouty concretions, which so largely consist of urates. As illustrative

however, of the occasional difficulty of diagnosis, he adduces two cases of gout which very closely simulated chronic rheumatic arthritis. To rheumatism, on the contrary, the disease is so intimately related, that the author is inclined to believe that it may be regarded as the more serious or malignant expression of the rheumatic diathesis.

His experience of the results of treatment is not more favorable than that of the majority of writers on the subject. To colds, draughts, moisture, and over-exhaustion, as injurious elements, he adds *absolute rest*, and he suggests the warm, dry, and equable climate of Upper Egypt as likely to be of service to the unhappy sufferers from this intractable malady. Alas! how few of them could find the ways and means of carrying this advice into effect.

Dr. George Asp, of Helsingfors, has investigated the "Mode of Termination of the Nerves in the Salivary Glands." His experiments were made on the submaxillary glands of dogs, rabbits, guinea-pigs, and rats; also on the lingual glands of frogs. As to the important question of the relations of the nerves to the gland-cells, the results were wholly negative, and consequently in opposition to the views advanced by Pflüger. The positive conclusions arrived at by the author were as follows:

(1) The salivary glands are not racemose, but tubular; or, if the expression is preferred, they form a transition between these two kinds of gland.

(2) The glandular tubes are lined by a continuous membrane; they divide dichotomously, interlace in all directions, and terminate in somewhat dilated *culs-de-sac*.

(3) The "salivary tubes" (Speichelröhren) of Pflüger are nothing more than the inferior parts of these *tubuli* passing without interruption into the terminal sinuses. The point of transition is lined with a double layer of oval or spindle epithelial cells, with large defined nuclei, surrounded by a smaller mass of finely granular protoplasm. This space is short, and the dichotomous bifurcation of the tubules takes place at its termination.

(4) The so-called "lunula" in the "alveoli" of the salivary glands is formed of cells analogous to those described by Heidenhain under the name of "Belegzellen" (Rollett's "cellules délomorphes"), grouped in the pepsin-glands.

The second part of the volume commences with a monograph on "Infantile Cataracts," from the pen of Dr. C. J. Rossander. The author prefers this term to the more usual one of "Congenital Cataracts," because he believes it not to be sufficiently made out that the cataracts so described are such in the true sense of the word; and further, because those which arise in the first weeks of infancy so closely resemble congenital cataracts as to require to be dealt with simultaneously. Of "infantile cataracts" there are two great groups, the *total* and the *partial*, the former being subdivided into *nuclear* and *cortical*. The author then traces the course of these two forms of total cataract, and describes the changes undergone by the lens, the intracapsular cells, and the capsule itself. Lastly, he establishes the relations of the cataract to the suspensory ligament of the lens (Zonula



Zinnii). The complications, including *amblyopia*, are treated of. After operation, which succeeds best in persons a little advanced in age, he has found the field of vision constantly very restricted, and notions of the distance and depth of objects always at fault. The state of the patients may improve, and often does so, but very slowly. Frequently those who enjoy good sight for a longer or shorter time ultimately become blind.

As regards treatment, in liquid and soft cataracts he punctures with a flat needle. In the case of cataracts which are of a greater consistence he prefers *discision*, or the modified extraction of Von Graefe. When operating on cataracts complicated by total posterior synechia, he adopts the method which consists in making a preliminary iridectomy inferiorly, and, some time afterwards, iridectomy and linear extraction superiorly. Details of the manner of carrying out the various operations are given.

Among *partial cataracts* he describes *cataracta polaris anterior* ("pyramidal cataract"), and *cataracta stratiformis* ("Schichtstaar" of the Germans, or "zonular cataract"). For the latter, when the diameter is not more than three lines, he recommends *coremorphism*, but only by means of iridectomy. *Iridodesis* is rejected as a bad operation. When the diameter exceeds three lines *coremorphism* is to be practised (if at all) only as a preparation for extraction or *discision*. It is impossible to do justice to this paper in such a (necessarily) brief notice as the present.

Chr. Fenger, Prosector at the Kommune-Hospital of Copenhagen, contributes a memoir on "Partial Hydronephrosis," and illustrates his remarks by the following clinical history:—A young man, aged twenty, was attacked by perityphlitis, with a congestive abscess in the right iliac fossa and thigh. In the course of the disease a round fluctuating tumour appeared in the region of the right kidney. This tumour one day suddenly vanished, to reappear after some time. An exploratory puncture subsequently made gave exit to urine, thus revealing the existence of a hydronephrosis. Six months after the onset of the malady vast abscesses caused the patient's death. The upper third of the right kidney was transformed into a sac as large as a child's head, separated from the pelvis, which lay inferiorly, and was slightly dilated by a movable valve formed by the normal partition between two calyces involved in the hydronephrosis. This valve owed its origin to a compression of the ureter by the perityphlitic abscess, which resulted in a moderate dilatation of the pelvis. In the upper part of the latter one of the partitions longer than the rest, in the form of a valve, became opposed to the adjoining wall, thus separating the upper third from the remainder of the pelvis. Subsequently this developed in less than six months into a complete hydronephrotic sac. The surrounding renal structure underwent such a degree of atrophy that only a layer of it one millimètre in thickness remained on the inner wall of the sac. The latter contained 750 cubic centimètres of a light yellow clear fluid.

The third number of the volume contains a "Statistical Report, with Clinical Observations, from the Stockholm General Maternity

(Barnbördshus), on Puerperal Inflammations in and about the Uterus." The author of this paper is Professor A. Anderson. In the five years ending 1870, 2641 lying-in women were treated in the maternity; of these, 1175 (44·5 per cent.) were primiparæ, and 1466 (55·5 per cent.) pluriparæ. The cases of uterine and peri-uterine inflammations numbered 303 (11·5 per cent.), including 199 primiparæ (65·7 per cent. of those attacked), and 104 pluriparæ (34·3 per cent.). Of the primiparæ, accordingly, 16·9 per cent. were attacked, and of the pluriparæ only 7·1 per cent. The total deaths were 95, but of these 16 have to be eliminated; the remaining 79 deaths were caused by various forms of puerperal fever, 48 of the patients being primiparæ and 31 pluriparæ. As a very common and easily recognised sign of uterine or peri-uterine inflammations, the author mentions a diffused hardness, sensible to the touch, in one or both of the iliac or inguinal regions. In the latter case the whole hypogastrium is invaded by a continuous tumour. At other times a round and more limited swelling exists in the neighbourhood of one of the Fallopian tubes. The pathological causes of these tumefactions are described at length. The fact that clinical phenomena in the puerperal state so often point to one *cornu* of the uterus as the seat for uterine and peri-uterine inflammations is easily explained when we reflect that the placenta is most frequently attached in this situation, and that the placental attachment is one of the most potent exciting causes of puerperal affections. Dr. Anderson analyses the morbid changes discovered on internal examination of the genital organs in these affections, and sketches the differential diagnosis of parametritis and perimetritis. Among the displacements of the womb produced by peri-uterine inflammations, latero-version is most usually observed; next in order of frequency comes ante-version, and lastly, retro-version. The important rôle played by infection in the spread of puerperal inflammations is proved by the fact that these are much more common in the maternity than outside, and that they prevail at certain epochs. Traumatism is an important exciting cause also. It is necessary to admit the existence of a special cause—infection; to its influence traumatism lends a helping hand.

In the third number, in addition to the papers already mentioned, we find "A Report on 422 autopsies performed at the Kommune (Municipal) Hospital of Copenhagen, in the year ending September, 1872," from the pen of Chr. Fenger. Dr. Adam Öwre (Christiania), offers an additional contribution towards the solving of the question, "From whom is syphilis inherited?" (cf. last year's Report, p. 219); and Dr. E. J. Bonsdorff (Helsingfors) writes on the treatment of croup. He lays down two principal indications to be fulfilled in dealing with this formidable malady; *first*, to diminish the plasticity of the blood; *secondly*, to remove the croupal exudation. The first indication is met by the administration of tartarated antimony, in weak but repeated doses (1 or 2 centigrammes), combined or not with iodide of potassium (10 to 20 centigrammes). The same remedy, but in quantities sufficient to induce vomiting, fulfils the second indication. When this fails, the author cauterizes the throat freely with a very strong solution of nitrate of silver. He uses a large camel's hair



brush. Having moistened it in water, he dips it into dry powdered nitrate of silver, and carries it over the posterior wall of the pharynx, the epiglottis, and neighbouring parts. He then induces vomiting by tickling the velum palati with the brush freed of the salt. In twenty years Dr. Bonsdorff has treated more than seventy cases of croup without losing a single patient, or ever having had recourse to tracheotomy. The present paper contains the clinical account of six cases of the affection.

“On the Operative Treatment of *Mus Articuli* (Loose Bodies in Joints”) is the title of an article by Axel Iversen, Consulting Surgeon (Reservekirurg) to the Municipal Hospital, Copenhagen. In his work the author extols the extraction of articular bodies by the antiseptic method, an operation which he prefers to the palliative measures of *compression* (by Middleton’s bandage), *acupuncture* (after Jobert or after Dieffenbach), *scarification of the synovial membrane* (Dufresne and Chassaignac), *subcutaneous ligature* (Dumoulin), *fixation* by means of a large *serre-fine* (Wolf). Of the radical measures of direct and subcutaneous incision, he selects the former, provided it is carried out after the antiseptic plan. The patient should be in his own bed. No chloroform is required. The whole operation should be performed under the carbolic douche. The instruments, the surgeon’s hands, the compresses, &c., should be moistened with carbolic solution (strength, 1 part in 50). The limb should be secured by means of strips of carbolized gauze, applied above and below the knee, to a suitable splint lined and covered with the same gauze. The foreign body is fixed by an assistant, if possible, against the external condyle of the thigh, and is then cut down upon by layers, the skin being first drawn well to one side. The wound is brought together by carbolized silk sutures, a piece of carbolized gauze is laid over the entire knee, folded eight times, with a piece of gutta-percha paper between the external layer and the next. Lastly, the whole is fastened to the splint by means of a carbolized bandage. The limb is placed in a *gouttière*, a bladder of ice is suspended above the knee, and the dressing is left undisturbed for from six to eight days. When it is renewed the same antiseptic precautions are adopted. Union generally takes place by first intention. Thirteen cases are cited by the author in illustration of the results of this method; of these, 6 were operated upon by Professor Saxtorph, at the Frederik-Hospital; and 7 by M. Holmer, surgeon-in-chief to the Municipal Hospital. One case proved fatal; perfect and uncomplicated recovery followed in all the others.

Dr. Christian Lovén (Stockholm) gives the results of a series of experiments undertaken with a view to determine the existence of lymphatic sinuses in the mucous membrane of the stomach. He employed the ordinary cold blue injection-fluid, a precipitate of ferrous sulphate and red prussiate of potash in water. To this he has latterly added a little perosmic acid. He also used the corresponding precipitate of cupric sulphate and yellow prussiate of potash—cupric-ferrocyanide. By injections with these fluids he has demonstrated pre-existent lymphatic canals in the gastric mucous membrane of man (children and adults), the dog, cat, calf, sheep, and rabbit. The

arrangement of these vessels varies somewhat in the animals mentioned, but in general it is possible to distinguish three layers in the lymphatic apparatus of the mucous membrane. Two of the plexuses of vessels are arranged parallel to the surface, and a third is formed by vertical intermediary canals. The *superficial* plexus lies immediately beneath the surface of the mucous membrane. The vertical canals, for which the author proposes the name of *interglandular lymphatic sinuses*, pass down more or less directly in the intervals between the glands. They communicate with each other by lateral branches, sometimes numerous, sometimes few in number, and terminate in the second or *subglandular* plexus. This lies immediately above the muscular layer of the mucous membrane, beneath and around the inferior extremities of the glands. By short canals perforating the muscular layer this plexus communicates with the *submucous* plexus, formed of generally large canals, which give origin to the efferent lymphatic vessels, provided with valves. Three splendidly executed and coloured lithographs illustrate this monograph.

There are many other valuable papers in this volume, but we have space to give little more than the headings of them. Under the title "Specimens of Tumours from the Pathological Institute of Lund," Dr. M. V. Odenius reports several interesting cases. The first was a case of carcinoma of the stomach, with secondary dissemination upon the peritoneum, in a labourer, aged 52. The illness lasted 7 months. The second preparation, one of soft subserous lipoma of the mesentery, was from a fisherman, aged 55, admitted to Malmö Hospital on March 16th, 1869, with peritonitis and ileus. A third specimen, hard subserous fibroma of the transverse colon, was without clinical history. The fourth was a remarkable example of chondrosarcoma of the thigh from a girl, aged 17. The limb was amputated 6 months after the appearance of the first symptoms. Pyæmia proved fatal 15 days afterwards, and metastatic abscesses were found in the lungs at the autopsy.

Mr. Franklin Nyrop (Copenhagen) relates a case of inversion of the uterus in a virgin, aged 50, depending on the presence of a sarcoma of the fundus.

Chr. Fenger, who has been already quoted, communicates a remarkable example of stenosis of the orifice of the pulmonary artery and of the artery itself in a boy, aged 11. The cause of the lesion was the growth of large vegetations on the semilunar valves and in the artery. The *post-mortem* conditions were—endocarditis of the pulmonary, tricuspid, and aortic valves, polypoid endarteritis of the pulmonary artery with stenosis, hypertrophy of the right heart, infarction of the right lung, croupous pneumonia of the left lung, engorgement of the spleen, diffuse peritonitis, parenchymatous nephritis, meningitis of the right hemisphere of the brain! Small wonder that the patient died!

In "Annotations on Massage" Drs. G. Berghman and U. Helleday, who had assisted Doctor Mezger at Amsterdam for three months, mention a case of peripheral but complete paralysis of all the muscles of the forearm and hand, in which electricity had failed to evoke muscular contractions, and which was treated by *massage*. At the



first *séance* slight muscular movements were observed, and in three weeks the patient was cured by *massage* alone, without electricity.

Dr. Ditlevsen (Copenhagen) writes on the structure of tendon-tissue, with special reference to the results obtained by the chloride of gold treatment. His paper is illustrated by a plate containing 8 lithographic drawings.

In an article on "Wounds entailing Danger to Life, from a Medico-legal point of view," Professor J. A. Estlander (Helsingfors) shows the necessity which exists for adopting a system of legal medicine which shall determine the significance of the phrase "dangerous to life" as applied to wounds, and the true meaning of the term "wound."

Through the last three parts of the volume runs a treatise by Dr. Curt Wallis (Stockholm) on the "Winter Resorts of Sicily." It is much to be desired that this distinguished climatologist, who last year also published an account of the climate of Egypt in the '*Hygiea*,' should shortly reproduce these and his other writings on the same subject in a permanent and compact form.

Dr. Jacob Heiberg, of Christiania, contributes a memoir on "Morbid Processes in the Cornea."

In a paper on the "Local Treatment of Chronic Gonorrhœa and Gonorrhœal Rheumatism with the aid of the Endoscope" Dr. Fenger describes an improved form of the instrument. The various affections which accompany "chronic gonorrhœa" are—superficial inflammation of the mucous membrane in the posterior part of the urethra, parenchymatous or hyperplastic local urethritis, granular urethritis, herpetic urethritis, condylomata, polypi, primary syphilitic ulcer, and secondary syphilitic eruptions (the last of very rare occurrence). He believes that the most usual cause of gonorrhœal rheumatism is a chronic local urethritis, and he maintains the rheumatism will not yield to any treatment so long as the affection of the urethra persists.

The remaining papers in this volume are one by J. A. Florin (Helsingfors) on "Medico-legal Systems, and the application of the 'Danger-to-life' principle as a test of the severity of Wounds," and a *brochure* by Professor J. A. Estlander (Helsingfors) on "Spontaneous Septicæmia."

It is fitting to mention here that we are indebted largely to the admirable "Quarterly Reports on Northern Medical Literature," which are appended to each number of the '*Nordiskt Medicinskt Archiv*,' for the substance of several of the abstracts of papers which follow.

2. In the January number of the '*Hygiea*' for 1873 Dr. E. Braun describes a case of eversion of the bladder, with separation of the symphysis pubis, the subject of the malformation being a girl 14 days old. A red, fleshy, moist and glistening tumour, the shape and size of an egg, was elevated above the surface of the skin in the epigastrium: it proved to be the everted urinary bladder. The cervix and urethra were completely absent, while the urine was observed to trickle from the orifices of the ureters in the lower border of the tumour. Immediately beneath these were two openings into the vagina, set obliquely to each other. The labia majora were present,

but the labia minora and clitoris were wanting. The anus lay 3·5 centimètres behind the orifice of the vagina. The pubic bones were separated by an interval of 4·5 centimètres, and in the position of the symphysis a thick, strong, and flexible ligament could be felt through the integuments. This ligament separated the tumour from the vaginal aperture. The child died of left pneumonia when about a month old. On *post-mortem* examination it was observed that the uterus was bicornate. In the same number of the journal Dr. Piscator writes on the "Endemic Trachoma of Tryksdalen," a valley of Wermland. In this valley a trachomatous conjunctivitis has prevailed locally for more than forty years without any apparent cause; indeed, the usual etiological elements are singularly wanting.

In the February number Dr. C. Edling communicates a case of abortion, which is of considerable medico-legal importance. An unmarried girl applied to a person of ill-repute to induce abortion. It appeared that this miscreant introduced a reed into the vagina, and through it blew up some substance which was presumably arsenic. "White grains," or "shining crumbs" (as they were described), came out of the vagina the same evening. The unfortunate girl at once became so ill as to be obliged to take to her bed. She died in a week. Blood and bloody mucus were found about the external genitals, but there was no trace of external violence. The uterus was 8½ centimètres long, by 5 broad. A sanguineous mucus lay in the os, and in the cavity of the womb, to the upper part of the posterior wall, a brownish-red, fleshy mass, as large as a walnut, was attached. On chemical analysis arsenic was found in the uterus, but not in the stomach. The next paper contains an account of poisoning by the external use of arsenic, by Dr. P. A. Levin. Three farm-servants got scabies, and a shepherd gave them an "itch-ointment" to cure it. Very alarming symptoms followed its application, and one of the patients fell into a typhoid state, from which he rallied only to become ataxic. Ultimately all recovered. The shepherd said the "salve" was composed of soap and gunpowder, with some *mercury*. The mercury proved to be arsenic, of which a mass the size of a horse-bean was present! It is needless to say that the scabies was radically cured. C. Cederström makes an official report to the Royal College of Health on two cases, in which toxic symptoms appeared after a sting in the finger inflicted by the fish *Trachinus Draco*, the sea-dragon.

Professor C. Santesson contributes to the March number the full clinical history of a large carcinomatous cystoma of the right testicle in a man, aged 23. The tumour was removed on November 2, 1872, and on December 30 following the patient died from pulmonary complications. Dr. C. Blix examined the mass microscopically. In the interval between innumerable small cysts, small, partly round, partly oval, sharply defined indurations appeared, of a greyish-white or reddish-grey colour and loose consistence, in which were found extremely small irregular cavities. Examined with the microscope, these indurations showed a typically carcinomatous structure, with great abundance of blood-vessels, out of which bleeding was going on in places, contributing to the appearance of the indurations. The



small cavities had no independent walls, but were the result of partial fatty degeneration and breaking down of the cancer-tissue.

The June number contains a successful case of resection of the knee-joint by Seved Ribbing. It was remarkable chiefly for the extreme difficulty experienced during the operation in straightening the strongly flexed limb; subcutaneous tenotomy failed, and an additional slice of bone had to be removed from the femur and tibia; the patella was also taken away. Notwithstanding the unfavorable condition of the soft parts, a slow but admirable recovery followed, the limb being put up in Petit's tin splint and dressed with Lister's carbolic paste.

In the same number a terrible case of poisoning by arsenical anilined at Malmö is reported by Dr. A. Jäderholm. After supper, on January 26, 1873, a man, his wife, and three children, were all suddenly seized with headache, vomiting, and intense pain in the belly, with collapse in some of the cases. The children all died. It was found that the wife had been colouring woollen yarn with anilin-red in a stew-pot, in which she had afterwards cooked potatoes. The peel of the potatoes was stained by the dye, and they contained arsenic, as did also the wool. An analysis of the dye yielded about 6 per cent. of white arsenic. The bodies of the children contained arsenic, but no anilin. Autopsies were made most carefully by Dr. Falk, and of these a full account is given.

In the July number Professor C. J. Rossander details a case of writer's cramp, cured by massage and subcutaneous injections of strychnia. A translation is to be found in the 'Irish Hospital Gazette,' October 1, 1873, p. 294. Professor C. Santesson also gives a detailed account of a large round-celled sarcoma in a lad, aged 16, which sprang from the periosteum and was undergoing ossification. The swelling engaged the greatest part of the right scapula, which, indeed, lay imbedded in, or "set in," the mass. It was extirpated by resection of most of the scapula—only the acromion, coracoid process and glenoid cavity, being left, the shoulder-joint being thus preserved in its integrity. The patient recovered perfectly. Dr. Blix describes the pathological anatomy of the tumour and removed portion of bone.

Last year a short notice was given of Dr. O. F. Hallin's report "On the State of Hospitals in Sweden during 1870." A similar abstract of his report for 1871, which appeared in the August number of the 'Hygiea' for 1873, may be of interest. At the beginning of 1871 there were 60 hospitals and *kurhusen* in the kingdom, and at the close of the year the available beds numbered 3360. In the provincial hospitals (analogous to our *county infirmaries*) 15,720 patients were treated, of whom 13,017 recovered or were relieved, and 1207 died (giving a mortality of 7·6 per cent.). The remainder were in hospital on January 1, 1872. In the *kurhusen* there were 62 deaths, and 5517 patients came under treatment for *venereal* diseases. In the military hospitals 422 persons were admitted for these complaints, the total number, therefore, being 5939. The number of cases in 1870 had been much higher—namely, 7441.

The remaining numbers of the volume for 1873 are taken up with "Pharmacological Communications from the Vienna International

Exhibition," by Dr. Oskar Th. Sandahl (September); a treatise on the "Climate of Egypt," by Dr. Curt Wallis; and a "Report of the Third International Medical Congress at Vienna in 1873," by Dr. R. Kullberg. "An Extract of the Yearly Report of the Surgical Division of the Royal Seraphim Hospital," by Professor C. Santesson, in the October number, should not be omitted.

We cannot conclude this notice of the 'Hygiea' without a reference to Dr. A. H. Wistrand's "Review of the Morbidity of Sweden in 1871." A melancholy interest attaches to this review, for it has proved to be the last which its talented author was spared to write. On January 10, 1874, Dr. Wistrand died at the comparatively early age of 55. Since 1850 he had devoted himself to medical jurisprudence, and since 1861 he was Extraordinary Professor of Medical Jurisprudence and State Medicine at the Caroline Institute, Stockholm. The health of the people showed a decided improvement in 1871, mainly owing, as Dr. Wistrand thinks, to a succession of good harvests. An epidemic of measles, which had attacked about 40,000 persons in 1869, and 2492 persons in 1870, almost died out—the cases in 1871 being only 676, with 39 deaths. Typhus and "gastric" fevers diminished (12,935 cases and 1034 deaths, compared with 20,470 cases and 1881 deaths in 1870). The following epidemic affections also decreased in frequency—smallpox, scarlatina, whooping-cough, and diphtheria. On the other hand, the column devoted to cholera, cholerae, and diarrhoea, showed a slight increase. Of Asiatic cholera 35 individuals sickened and 21 died. Inconsiderable local epidemics appeared at Göteborg and the surrounding district, and in the neighbourhood of Hernösand. The latter outbreak was clearly caused by importation from Vasa, in Finland.

3. The 'Norsk Magazine' for 1873 contains many papers, which we cannot notice at present. Among others, several on *massage*, a method of treatment which seems to have attracted much attention in Norway, and which was described in our 'Review' of last year (p. 231, *seq.*). In one of these articles Dr. C. Eger, of Arendal, recalls two old communications on massage—one in the 'Comptes Rendus Hebd. de l'Académie des Sciences,' 1857, and the second in the 'Medical Times and Gazette,' No. 395, 1858. Truly, "there is nothing new under the sun."

A. Abel records an example of several congenital defects in the same family. A man who married his cousin had 10 children in all, of whom two died young. Four were born with 6 toes on each foot and 6 fingers on each hand. Three resembled their father and paternal uncle in being squint-eyed. Two of the sons (both having supernumerary fingers) were idiots. Three of the father's and mother's common cousins, who are now dead, had each 12 fingers and 12 toes. Leprosy (Spedalskhed) also prevailed in the family.

W. Dietrichsön reports to the Department of the Interior an epidemic outbreak of a peculiar fever at Trondhjem in 1872. It was characterised by its sudden invasion, severe pyrexia, pains in the limbs, violent headache, vertigo and considerable debility, cardialgia, followed by nausea, frequent vomitings, complete anorexia, sometimes diarrhoea,



sometimes constipation. The lymphatic glands in the neck, and occasionally those in the inguinal and cubital regions swelled and sometimes suppurated. The tonsils were inflamed and often coated with a diphtheritic exudation. The disease ran a course of from 8 to 14 days. The cause was the ingestion of bad drinking water, due to the pollution of the streams in an unusually dry summer, and a subsequent "fresh" from heavy autumn rains. The malady appears to have been a modified form of enteric fever.

Jacob Heiberg contributes an interesting account of two fatal cases of malignant pustule, produced from inoculation from cows which had died of splenitis. A microscopical section through one of the pustules showed that the epidermis and underlying corium were shrivelled and somewhat granular, tinged brown with blood-pigment. At the border of this dried-up portion, and just beneath the epidermis, lay a finely granular mass, insoluble in caustic potash or acetic acid, which proved to be a collection of spheroidal bacteria. The papillæ and corium surrounding the dried-up portion were infiltrated with numerous round cells and a great many fine granules.

In a "Fragment from Lectures on Diseases of the Circulatory System" Dr. Larsen describes and details six cases of what Virchow termed "malignant endocarditis," the "acute destructive endocarditis" of R. Meyers. In only one of the cases was the diagnosis made with any degree of certainty during life. In four, fungoid growths were found in the heart; and in two, similar forms also existed in the peripheral lesions—cutaneous ecchymoses or pustules. On the endocardium a deposit of a soft, greyish substance, consisting of connective tissue, pus, and detritus, takes place. There are ulcerations; and abscesses, ruptures, and perforations, occur in the subjacent muscular structure. The disease is located most frequently in the mitral orifice, more rarely in the aortic or tricuspid valves. Abscesses and infarctions are met with in all the peripheral organs; occasionally embolism with anæmia in the peripheral vessels.

Under the title "Nosogeni-Panspermi," Professor Fayé reviews most ably the existing literature of the germ theory of disease.

Microscopic organisms have been discovered in intermittent fever, cholera, dysentery, variola, vaccinia, morbilli, scarlatina, diphtheria, tussis convulsiva, blennorrhagia, pyæmia, and septicæmia, puerperal fever, tuberculosis, and, as is well known, in several skin affections. This admirable summary has been lately reprinted in pamphlet form.

From the Medical Division of the Royal Hospital (Christiania) Professor Winge reports a case of aneurism of the innominate artery in a married woman, æt. 38, who ten years previously had had syphilis. The sac, which was of enormous size, engaged the innominate trunk alone, but had eroded the bodies of the vertebræ as well as the clavicle and first rib on the right side.

In a lecture on Diseases of the Mind Dr. P. Winge, alluding to Laillier's researches as to the occurrence of diabetes mellitus in the insane, states that his investigations made in the Christiania Asylum go to prove that sugar is tolerably frequently present in the urine of lunatics, at least in men, although seldom in large quantity. The

experiments, however, were made only on male patients, in 16 per cent. of whom sugar was found.

Dr. Edward Bull details a case of "Symmetrical Gangrene in the Extremities," an affection first described by Raynaud in 1862. The patient was a servant-girl, 18 years of age, who had always enjoyed good health. The catamenia, however, had never been quite regular, and had ceased for two or three months. Itching began in the fingers of both hands, which became purple and swollen. Severe intermittent pain prevented sleep, but her general health was only somewhat disturbed. The treatment was expectant, morphia being given to allay pain. The gangrene went on, and cataplasms were applied, under which the gangrenous, foetid parts separated and the pains ceased. In the course of two months the sores healed, leaving so deep a cicatrix that the greatest part of the last phalanx of the little finger, on its palmar aspect, was destroyed.

Engvald Hansen, of Stavanger, records a novel application of electricity. On June 3rd, 1873, he was called to see a little girl, 7 weeks old, whom he found in a state of asphyxia from intense capillary bronchitis. The infant did not respond to the stimulus of cold water or tickling the fauces, and was in fact moribund. With an induction-machine Dr. Hansen proceeded to faradize the phrenic nerves rhythmically, each incitation lasting about two seconds only, but the current being very strong. At first there was no reaction, but ultimately the stimulus began to work, the child gradually breathed better and better, the face became more life-like, the limbs moved, and finally crying and cough set in. The electricity was then given up, the infant was allowed to inhale steam, and was laid in a steam-press. A speedy and complete recovery ensued. The author points out that the employment of electricity as a *dernier ressort* in such desperate cases is not new. Ziemssen, Abelin, and Kjellberg had recorded striking examples of the utility of the remedy.

In the December number S. Höegh, of Molde, narrates a case of epilepsy in a girl, æt. 23, in which he used nitrite of amyl inhalation, as recommended by Dr. J. Crichton Browne. The result was very unfavorable; the attacks, which had been 1 by night and 1 by day, increased in frequency until they numbered 3 to 5 by night and 2 to 3 by day. "This," says Dr. Höegh, "is only an isolated case, but it stands in such direct contrast to Dr. Browne's emphatic recommendation of nitrite of amyl that I have made it known at once." The quantity inhaled was 6 drops four times a day.

4. In the 'Ugeskrift for Læger,' vol. xv, No. 25 (May 24th, 1873), Dr. Hirschsprung communicates a case of diabetes mellitus in a girl, æt. 8. During 303 days' sojourn in hospital she excreted 28,470 grammes of sugar (about 63 pounds), and lost a little over one quarter of her previous weight. The daily quantity of sugar was least under Bouchardat's (meat diet) treatment, and greatest under the arsenic method. A very full abstract of this paper, and of two similar communications by Drs. V. Budde and Schouboe, appeared in the 'London Medical Record,' Nos. 59 and 60 (February 18th and 25th, 1874). Dr. J. Jónassen writes on "Echinococci and their



Treatment." He illustrates his remarks by four clinical histories. Two patients died, two recovered. His treatment is by puncture with dilatation by means of the sponge-tent. Aspiration is not satisfactory, as the canula gets choked up with the hydatid cysts. Dr. Christensen (No. 27) has employed nitrous oxide gas extensively as an anæsthetic in ophthalmic operations of all degrees of severity, the youngest patient being a girl of 3, the oldest a woman of 76. Both suffered from double cataract. Deep narcosis was generally caused in about a minute, or after a couple of deep inhalations in children, and the effect lasted from  $1\frac{1}{2}$  to 2 minutes.

In No. 30 F. F. Ulrik describes an apparatus for use in writing in scriveners' palsy. It is sufficiently simple, consisting of a cylindrical piece of wood six inches in length and an inch in diameter, to which is attached at right angles a bent penholder about two inches long. The cylinder is laid across the palm of the hand, and the penholder projects between the middle and little fingers. The apparatus is easily used, and the irritated muscles remain in a state of repose.

Under date August 23rd, 1873 (vol. xvi, Nos. 9, 10), we find Dr. P. A. Schleisner's "Report of the Health of Copenhagen in 1872." An abstract of this valuable report may be found in the 'Irish Hospital Gazette,' vol. i, p. 327. Suffice it to say here that the rate of *morbidity* was 184 per 1000 inhabitants, and the death-rate 24 per 1000. In No. 11 of this volume a case of poisoning by subcutaneous injection of morphia is recorded by Dr. A. Berg, of Nyköbing. A woman, æt. 48, who was accustomed to take morphia internally for heart and bronchial symptoms, had an attack of rheumatic lumbago. Dr. Berg accordingly injected in the lumbar region not more than an *eighth of a grain* of morphia. Sharp symptoms of opium poisoning followed—severe vomiting, languor, and thirst, the pupils contracted and very sluggish, the skin pallid and cold, lips blue, pulse weak (64), and breathing somewhat irregular. Stimulants and counter-irritation at last brought her round. Dr. Berg thinks the morphia must have entered a vein.

Two other articles in this volume are well worthy of notice, although we cannot hope to do them full justice. The first is by Dr. V. Budde (in No. 21) on "Peculiar Stethoscopic Conditions in Pleuritis and Pneumonia in Children," illustrated by two clinical histories. Dr. Budde points out the unsatisfactory nature of the results yielded in the case of children by the usual signs relied on for the differential diagnosis of pleuritic effusion and pneumonia, and lays stress upon a most valuable physical sign first observed by Dr. Hirschsprung. That distinguished clinical physician has constantly found in empyema in children, even where the pleura is quite full of fluid (and perhaps most notably so in that case), that the percussion sound is semi-clear, or tympanitic, over an area of one or two fingers in width along the spinal column from the base up to the root of the lung. Dr. Budde is convinced that this sign is present in slight effusions also, and as catarrhal pneumonia produces complete dulness in the situation mentioned, the presence of this form of tympanitic dulness is *quam proxime* pathognomonic of pleuritis with effusion. The explanations

of the phenomenon given by the two observers are essentially different. Dr. Hirschsprung holds that it is produced by the expanded healthy lung, and this is the opinion entertained by Dr. Walshe, who, indeed, noticed the sign, but spoke of it only cursorily. Dr. Budde, on the other hand, maintains that it proceeds from the compressed lung, the remaining elasticity of which allows it to vibrate under the influence of external percussion, and so to transmit a tympanitic note.

The second paper (in No. 22), on "Retroflexion of the Uterus in its relations to Conception and Pregnancy," is by Dr. Stadfeldt, the Professor of Midwifery at Copenhagen, whose 'Lectures on Midwifery' were reviewed in last year's report. From his observations he believes that we should be cautious in seeking for a cause of sterility in existing retroflexion of the womb, although we must regard this malposition as an essential hindrance to conception. "I repeat," he says, "that retroflexion may accompany sterility, but it is far from producing it in all cases." His statistics embrace 36 cases of at least right-angled retroflexion, where no mechanical treatment had been adopted. In 8 of these women he had seen pregnancy occur within two years. He had traced down the cases from the beginning of pregnancy; in 7 he had recognised the existence of retroflexion before conception, and *in all* again after delivery. Of the rest (28) only 7 had been sterile in two years. He remarks—"It appears to me that this experience stands in direct contrast to the current teaching of the day."

5. We have received but four numbers of the 'Upsala Proceedings.' In the first of these Dr. Edward Clason and Hr. Hammarsten give a minute account of an intestinal infarction from a middle-aged woman, who had suffered from a retro-peritoneal abscess for some months previously. It consisted largely of mucus. In the second number Fr. Björnström details three cases of "Agoraphobia," an affection so named by Professor Westphal,<sup>1</sup> and which consists essentially in a singular dread of crossing a market-place (*ἀγορῇ*), street, or any large open space. All the patients were men. The third number contains a minute account, by P. Hedenius, of a case of diffuse phlegmonous œsophagitis observed by F. A. O. Belfrage in a married woman, æt. 42, who believed she had swallowed a fish-bone. The most prominent symptoms were shivering and pyrexia, violent pain and distress in the epigastrium radiating to the left side and back, nausea, but no vomiting, and (curiously enough) no difficulty or pain in swallowing. She died on the fourth day, and the *post-mortem* revealed a thickening in the lower part of the œsophagus, depending on a purulent infiltration into the submucous tissue, without a trace of ulceration of the mucous membrane or any lesion referable to the swallowing of a fish-bone. The morbid process extended through the cardiac orifice into the cavity of the stomach, which presented a similar thickening at the lesser curvature over an area of 8 centimètres in extent.

In the third number O. V. Petersson reports two cases of *Balanti-*

<sup>1</sup> 'Archiv für Psychiatrie und Nervenkrankheiten,' B. iii, Heft 1.



*dium Coli*,<sup>1</sup> which occurred in Professor Glas's *clinique* at the Academical Hospital, Upsala. Both patients were men, aged 46 years, and in both the prominent symptom was chronic diarrhœa. One survived, although much run down by a year's diarrhœa; the other succumbed after five months' diarrhœa. In the latter case, as in those already recorded, diphtheritic ulcerations were found throughout the large intestine, increasing downwards, and balantidia were present, but only below the valve of the colon. The author accordingly regards this intestinal affection as a form of chronic ulcerative colitis, with a catarrhal and an ulcerative stage, leaving it an open question whether the balantidium plays the part of a cause or of a complication. The symptoms are those of chronic colitis; the diagnosis, therefore, cannot be made without the aid of the microscope; prognosis is unfavorable, and treatment but little satisfactory in its results. Of the 10 cases hitherto recorded, 4 were met with in the hospital mentioned above.

In a paper on "Some Parasitic Fungi, and their bearing on the Doctrine of Infection," Professor P. Hedenius describes (1) the mature spores of *Ustilago carbo*; (2) teleuto-spores of *Puccinia graminis*, with its mycelium in the straw of wheat; (3) Aecidium-sacs of *Puccinia graminis*, filled with spores and surrounded by peridium-cells; (4) spores and mycelium of *Empusa muscæ*; (5) *Saprolegnia*, or *Achlya prolifera*; (6) the Zooglœa-form of *Micrococcus* (*Monas* of Ehrenberg), *Crepusculum*, and *Bacterium termo*; (7) *Bacterium termo* in a free motile state; (8) swarms of filamentary *Bacteria*; and (9) the *Leptothrix* variety of the last named. In addition to the morphological conditions of these lower organisms, the author considers the part they play in the processes of fermentation and putrefaction, and their pathological significance in connection with infectious diseases.

R. A. Wawrinsky contributes to the fourth number a paper on "Different Methods of Precipitating the Colouring Matter of the Blood from its Solutions." He has practised the following methods of detecting blood in urine:—(1) Almèn's modification of Heller's test, or the formation of hæmin-crystals from a phosphatic (hæmatin) precipitate; (2) Struve's process of precipitating the hæmatin by means of tannic acid; (3) Van Geun's and Gunning's method of precipitating it with acetate of zinc; (4) Tidy's<sup>2</sup> method of throwing down the albumen (and possibly with it the hæmatin) by means of carbolic acid; and (5) Sonnenschein's method of precipitation by sodium tungstate or molybdate. All the processes were satisfactory, but the most sensitive was that by acetate of zinc, by which hæmin-crystals were obtained from a dilution of 1 part of blood in 5000 parts of urine. Next in point of sensitiveness came the modified process of Almèn. The guaiacum test and the zincic acetate method seemed to be about equally good.

In the same number F. Björnström and O. Glas report two cases of "Pericystic Hæmatoma." While making a *post-mortem* at the

<sup>1</sup> See the "Report on Scandinavian Med." in this Journal for 1872, vol. I, p. 207.

<sup>2</sup> To M. Méhu would seem to belong the priority of introducing this test. Cf. a letter from Dr. W. G. Smith in the 'Brit. Med. Journ.,' June 4th, 1870, p. 492.

Upsala Central Hospital, Dr. Björnström found a collection of coagulated blood, as large as a hen's egg, lying between the posterior wall of the bladder and the peritoneum. It communicated above through an ulceration in the serous membrane, both with an abscess in the anterior wall of the abdomen and with the peritoneal cavity itself, which was the seat of a diffuse peritonitis. The hæmatoma was regarded as the cause of the inflammation. Glas's case had a more fortunate issue, for the blood-tumour which could be felt through the rectum lying around the neck of the bladder, and which caused retention, dissected its way upwards into the anterior abdominal wall, where it was evacuated by puncture, complete recovery following. In this instance, the patient being a lad of 14 years, the affection was caused by violent concussions during a ride. In the first case the cause was not clearly made out, but the patient, a man aged 31, practised onanism.

6. Through the two first numbers of the sixteenth volume of the 'Hospitals-Tidende' runs a paper by Dr. Howitz, on Ovariectomy. This paper contains the clinical histories of 11 completed ovariectomies, of 2 incomplete operations, and of 2 exploratory incisions. Among the 11 completed operations, 2 patients died, 9 recovered. The author dwells particularly on the position of the uterus in ovarian dropsy. In three of the recorded cases it was pressed high upwards. In one instance the cyst during rotation had, as it were, wrapped the ligament of the uterus around itself, and had become attached for some distance to it and to the uterus, this viscus accordingly being suspended above the symphysis. One of the cases in which an exploratory incision was made proved to be an example of circumscribed ascites. Dr. Howitz observes that the connection of the stump with the abdominal cicatrix may become loosened under many conditions, as, for example, pregnancy. Several of his patients who conceived subsequently, suffered from pain in the abdomen in the third or fourth month, and in one case there were even mild symptoms of peritonitis. In none did abortion ensue.

In the 'Hospital Gazette' for February 12, 1873, Dr. Vald. Rasmussen details a case of fatal hæmatemesis, from bursting of a small sacciform aneurism of the left coronary artery of the stomach, associated with a rodent gastric ulcer. The patient, a journeyman tanner, was 45 years of age, and had enjoyed good health until four weeks previous to his death, when he began to suffer from cardialgia. A few days before he died repeated attacks of hæmatemesis occurred, he became anæmic, and fell into collapse. About the middle of the lesser curvature of the stomach a perforating ulcer was discovered— $2\frac{1}{2}$  centimètres long and 1 centimètre wide. The ulcer was not funnel-shaped, it had a smooth hard border, formed of the thickened submucous layer. Close to its anterior edge there was a slight prominence the size of a pea, which proved to be a burst sacciform aneurism on a branch of the left coronary artery. The efferent vessel of the sac was considerably narrower than the afferent branch. Dr. Rasmussen, in accordance with Bamberger's theory as to the cause of cardialgia in these cases, is inclined to believe that the cardialgic pains first complained of four weeks before death depended on the eroding influence of the gastric juice on the twigs of the vagus, which everywhere accom-



pany the arteries of the stomach; that the artery had been simultaneously laid bare by the approach of the ulcer, and that the commencing development of the aneurism may be assigned to this period. Like all Rasmussen's writings, the paper is as suggestive as it is instructive. Axel Iversen has a capital article on amputations of the knee. He treats of the operation of disarticulation,—of amputation through the femur, as recommended by Gritti;<sup>1</sup> and of the so-called trans-condyloid amputation. Four cases in which this last method was followed are given from the *clinique* of the Municipal Hospital, Copenhagen. The author recommends a large anterior flap as well as a very large posterior flap. The patella should always be removed, a partial detachment of the synovial membrane is usually practised, and Butcher's saw is used. The indications for amputation are—(1) acute suppurative synovitis; (2) chronic fungoid, or carious arthritis, where excision of the knee-joint is inadmissible; (3) new growths involving either the integuments or the bones of the leg, and which render an amputation through the leg impossible; and (4) senile gangrene of the foot and leg. In the number for April 2nd Dr. F. Howitz narrates no fewer than 27 cases of movable kidney. As a very full abstract of his communication appeared in the 'London Medical Record,' August 20, 1873, it will suffice to mention here that *all* the patients were women, and that pregnancy seems to be a very potent exciting cause of the displacement of the kidney. Traumatic influences were at work in four instances. The *right* kidney was displaced on twenty-two occasions, the *left* on five only. The principal portions of a paper by Dr. Rasmussen on "Aspiration in Serous Hydrarthrosis of the Knee-joint," are to be found in the 'Irish Hospital Gazette,' June 16th, 1873. Under the title, "Communications from the Children's Hospital," Dr. H. Hirschsprung details a case of suspected cerebellar tumour with diphtheritic paralysis, in a boy aged  $3\frac{1}{2}$  years; and also one of hydronephrosis, depending on obstruction of both ureters by a urinary calculus, in a child of 5 months. The diagnosis in the latter case was made from the following group of phenomena:—while in a state of extreme unrest the child had repeatedly passed small calculi of uric acid; anuria succeeded, continuing for three or four days. The bladder was empty, but in the lumbar region there was an extensive area of dulness and percussion. In the number for May 28th Professor A. Brünniche narrates a case of purulent peritonitis cured by aspiration and drainage (the patient was a girl aged 6); and in that for July 23rd Dr. Wiberg (of Humble) gives one of right empyema cured by thoracentesis with aspiration and injection of iodine, in a lad aged 17. In the 'Gazette' of July 30th, and in the two following numbers, we find a very important paper by Dr. V. Schepelern, Consulting Physician to the Municipal Hospital, on a case of myelogenic lineal leucæmia, and four cases of pseudo-leucæmia. "Myelogenic leucæmia" is the affection first described by Professor Neumann, of Königsberg, in 1869.<sup>2</sup> "Pseudo-leucæmia" (Cohnheim, Müller and

<sup>1</sup> 'Annali Universali di Medici,' t. clxi, fere 481. Luglio, 1857.

<sup>2</sup> 'Archiv d. Heilk.,' xiii, 481. "Ein neuer Fall von Leukæmie, mit Erkrankung des Knochenmarkes."

Wunderlich) is the affection called "Adénie" by Trousseau, and known as "Hodgkin's Disease,"—a malady, according to Mosler, resembling true leucæmia in etiology, symptoms, course, and pathological anatomy—the only difference being that, in the former disease, the normal healthy relation between the white and red blood-corpuscles is maintained. The number for October 8th contains an article by Dr. Hirschsprung on the relation between hæmaturia, albuminuria, and uric acid deposits. The author confirms Dr. Kjellberg's observations as to the etiological significance of uric acid, in determining hæmorrhage or the presence of albumen. The last paper in the volume before us which we can notice is one by Dr. C. Studsgaard, on "Internal Œsophagotomy in Cicatricial Stricture." This operation, said by the author to have been first performed by Trélat in 1870 (a statement which, we think, requires to be qualified), was practised on a little girl, Helga R—, 8 years of age, who suffered from a rapidly tightening stricture about seven inches from the mouth. Five months previously she had swallowed in mistake for water a mouthful of a solution of lime and potash. She was wasting from starvation, when on September 16th, 1873, Dr. Studsgaard divided the stricture by means of an Œsophagotome designed by himself, and a wood-cut of which illustrates his paper. Pains in the stomach and back set in two hours after the operation, but were controlled by treatment. On the 29th, No. 27 stomach-bougie (Charrière's measurement) almost passed, whereas on the 2nd, No. 11 had with difficulty been pushed through. Her weight increased from 2 st. 9 lbs. at the time of admission, to 3 st. 2 lbs. just before the operation; and to 3 st. 6½ lbs. when discharged on the 6th of October.

7. The Medical Society of Christiania held twenty meetings in 1873. Among the more important communications were the following:—Embolism of the pulmonary artery, and the differential diagnosis between this lesion and rupture of the heart; the question of the importance of fungi as a cause of pyæmia and puerperal fever, and the contagiousness of the latter group of affections; medullary leucæmia, with an illustrative specimen; a typhus (? typhoid) epidemic in Aker, a suburb of Christiania; the mechanical treatment of lupus; total absence of the iris in both eyes of a man aged 22 (congenital); eclampsia in pregnancy and abortion; birth of a double child, &c. Very interesting discussions took place respecting the outbreak of fever in Aker, and the relations of germs to pyæmia. Dr. Bidentkap also detailed a case of oxalate of lime calculus in a man aged 70, who for twenty-five years had habitually used rhubarb as an aperient. O. Lind related the particulars of a similar case.

8. The Swedish Society of Physicians met weekly, according to custom, throughout the greater part of the year. A few of the headings of the communications can alone be given: Suicide by excision of the external genitals (Baron von Düben); *Pneumonia Migrans* (at the meeting of January 21st, Dr. Bruzelius described a form of pneumonia under this name, characterised by wandering from one part of the lung to another when resolution has taken place in the part first affected); fatty degeneration of the heart with papillary insufficiency



in a boy aged 13 (Dr. Bruzelius); interstitial hepatitis with icterus, and hæmorrhage from telangectases on the face (the same author); suppurative perinephritis (Professor Malmsten); poisoning by nitrobenzin (Dr. Edholm); partial hydronephrosis with double ureters (Prof. Key); *arsenismus chronicus* (Professor Malmsten); ulcerative endocarditis with *mycosis endocardii* (the same); *pyelo-nephritis calculosa* (Dr. Bruzelius); excision of the uterus, followed by fatal peritonitis (Dr. Norström); ulcer of the stomach, with sudden death from hæmatemesis, the patient being Baron K. E. L., aged 52 (Dr. Lemchen and Professor Key); double ankylosis of the hip-joint (Prof. Rossander). On April 8th an interesting discussion took place on morphia poisoning, and on May 20th thoracentesis was made the subject of an instructive debate.

*The existing Epidemic of Smallpox at Stockholm.* At the meeting of December 9th, 1873, Hr. Grähs, chief medical officer of health to the city, made a report on the outbreak of smallpox, which began early in October, especially in the South Catarina District, and rapidly increased in extent and frequency throughout November. In the nine weeks ending December 5th the cases under treatment numbered 291, of which 37, or 12 per cent., terminated fatally. From the 'Proceedings' for 1874 we glean information respecting the further progress of the epidemic up to February 7th, 1874; from October 4th, 1873, the total number of cases was 1426, with 316 deaths.

Week ending February 14th,—180 cases; 37 deaths.

      "      "      "      21st,—213      "      41

The epidemic is thus seen to be increasing. Since the foregoing was written later intelligence has come to hand. From the commencement of the epidemic to the 21st of March, 2701 cases of smallpox had been reported. Assuming the population of Stockholm at present to fall not much short of 150,000 (in 1863 it was 124,691, and in 1871 it had risen to 137,052), we see how serious is the present outbreak. At the height of the epidemic in London the deaths in one week reached only 288 in a population of 3,000,000; but 41 deaths in 150,000 is equal to 820 in 3,000,000; so that the Stockholm mortality is considerably more than double that in London in 1871. One of the most curious things about the present outbreak is, that it has been so long postponed. A reference to the *Reports on Scandinavian Medicine* in this Journal for July 1872 and 1873 will show that an epidemic of smallpox began in Copenhagen towards the close of 1871 and raged for many months, while Sweden remained tolerably free from the disease until October, 1873.

9. Dr. C. Lange has reprinted from the *Hospitals-Tidende* a very important contribution to our knowledge of chronic interstitial myelitis. It is with regret that we find ourselves unable to give a detailed analysis of this book, but the space at our command is limited, and we fear we have already transgressed those limits,

## REPORT ON SURGERY.

BY HENRY A. REEVES, F.R.C.S.E.,

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*Cases of Hæmatoma Pericysticum* ('Upsala läkarefören förhand,' viii, p. 348, 1873; and 'Schmidt's Jahrb.,' B. 189, 1873). Dr. Björnströrn reports a case of this affection occurring in a man, æt. 31, who was admitted into the Upsala Central Hospital on June 4th, 1868. As a child he was scrofulous, and had measles, whooping-cough, scarlatina, and in later years was subject to attacks of cold. He had syphilis three years before, and since the summer of 1867 had given evidence of mental disturbances. He had a strong tendency to masturbation. Since the end of March he suffered from fever, with pains and tension in the abdomen. For some time he was occasionally better and worse, and in the middle of April vomiting occurred and the strength of the patient failed, and on 26th April he died.

The post-mortem revealed unusual fulness of the vessels of the pia mater, œdema under the arachnoid, and injection of the hemispheres. The pericardium contained 120 grm. of yellow exudation. The heart was healthy; both its cavities contained fibrinous clots and cruor. There were pleuritic adhesions, and the inferior lobes of lung were œdematous. There were evidences of peritonitis, the peritoneum being injected, and the bowels held together by fibrinous exudation. No perforation of the bowel. Behind the bladder and under its peritoneal coat was found a blood-clot the size of a hen's egg. It was in a decomposing state. At the point of this clot the peritoneum was ulcerated, so that on pressure the inspissated contents of the degenerating clot flowed out into the peritoneal cavity. The ulceration of the peritoneum extended for an inch also on the anterior abdominal wall towards the right; the right rectus was discoloured and infiltrated with pus. Otherwise the bladder was not affected. Kidneys normal in structure, but congested; capsule easily detached; spleen small; capsular thickened; pulp softened. A vascular membrane covered the whole organ. Liver very small; left lobe little developed, and its parenchyma red-brown, and acini indistinct. Gall-bladder somewhat empty.

How the blood-effusion occurred is not easy to say. The only indication furnished by the history of the case is the circumstance that the patient onanised, and was restless and violent.

Dr. Björnströrn assumes that the tendency to congestion in the neighbourhood occasioned by his habit of masturbation predisposed to a rupture of the vessels, and that the impulse causing the rupture was brought about during his attacks of violence. It is, however, possible that the hæmorrhage found its way from the bladder under the peritoneum. It is also difficult to explain the abscess-formation which



was found under the peritoneum and anterior abdominal wall, whether this caused the peritonitis through perforation of the peritoneum, or whether the latter was occasioned by the blood extravasation, and whether the peritonitis existed before the abscess, and was its cause. In favour of the first assumption is the circumstance that the appearances characteristic of peritonitis did not exist at the beginning, but came on later.

Nothing is said of the functional power of the bladder, yet one would think that both the hæmatoma and the peritonitis must have limited it; it is probable that the catheter was found necessary, as is so common in peritonitis, and that no note was made of its use.

A case which helps to throw some light on the matter, and aid in the diagnosis, is contributed by Dr. O. Glas. The patient was a boy, æt. 14, who, from September 11th to October 12th, was treated in the medical wards of the Academic Hospital in Upsala. After a long ride, thirteen days before admission, he suffered from pains in the stomach and vomiting, which lasted several days, then constipation, which was soon followed by severe diarrhœa, but without hæmorrhage. Urination was difficult and painful. On examination there was found, above the symphysis, a considerable resistance with dulness on percussion and tension of the coverings; above and at both sides of this limited spot, which was oval in form, the percussion sounds were normal. A hard tumour could be felt on rectal examination, which occupied a large part of the lower pelvis and pressed back the anterior rectal wall. The anterior wall of the abdomen and the perinæum were sensitive to pressure. As the tumour was taken to be a distended bladder, catheterization was practised, but only a few drops of urine came away, partly through the instrument, partly at its sides, and shortly after the urine passed voluntary. The pains continued during the night, therefore Dr. Glas punctured above the symphysis with a curved trocar the next morning, and let out 20 cc. of a thick, blood-stained, fæcal-smelling fluid, which contained no urine. Afterwards the pains left, and the patient could empty his bladder without complaint, and the tumour, which could be felt through the rectum, had much diminished in size. In the evening a similar fluid came away through the canula, but the next morning only pus. From this time the improvement was rapid, the tumour almost entirely disappeared, and the punctured wound began to heal. On September 22nd diarrhœa again occurred, and on the 24th the tumour had again formed, although of less size than at first. The pains caused by it soon disappeared after the application of poultices, and also the tumour, so that on rectal examination, on October 7th, only a slight resistance was perceptible, and percussion above the symphysis was only slightly dull. Urination was not disturbed. On October 12th the patient was discharged, and when he was again examined, on October 18th, no trace of the tumour could be felt.

*Observations on the Spreading of Erysipelas migrans.* By Dr. LUDWIG PFLEGER ('Arch. f. Klin.-Chirurg.,' xiv, p. 532, 1872).—The author was induced to undertake this investigation by the opinion

repeatedly expressed by Billroth, that the apparently planless method of spreading of erysipelas migrans was really not so, but might be found to be governed by definite laws.

The paper contains the results of the observations of seventy cases during 1870-1. He prepared a great number of outline figures of the body as seen in front and back views, and sketched in every case the inflammatory redness from the beginning of the disease. He also indicated in these figures the sharpness of limitation, colour, swelling, and the conduct of neighbouring veins and lymphatics, so as to procure an accurate picture of the path which the inflammation takes on the surface of the skin.

A comparative study of those cases which affected large portions of the body and had sharply defined limits showed that, as regards the direction and rapidity of progress of the disease in different parts of the body, differences occurred, which are repeated in every individual case, so that erysipelas proceeding from the same parts of the body furnish similar drawings in different individuals; but also erysipelas proceeding from different spots of the body exhibit a similar condition of their boundaries on arriving at certain regions. Dr. Pfleger found that the progress of erysipelas, by pointed processes and broad extensions from the limits of the exanthem, proceeded in a definite direction in almost all cases. These processes lay much nearer the surface of the skin than the centripetal and centrifugal veins and lymphatics, over which the cutis was moveable. The redness of the processes was like that of the rest of the erysipelas, and they were sharply marked off from the normal skin, but most distinctly on the lateral parts of the body when the patients lay on their back. In these regions the skin bulges and appeared to be more tense.

As regards the *separate parts of the body*, the author observed that over the supra- and infra-clavicular regions, from the free margin of the trapezius, the most frequent and well-marked processes which were directed forwards and obliquely downwards and inwards, if the erysipelas proceeded from the back, and conversely—outwards and upwards—if it proceeded from the chest. Processes coming from the back towards the linea alba run horizontally at the level of the umbilicus; but from the navel they were directed downwards until they ran parallel with the groin. Under Poupart's ligament they ran parallel with it, others inclined downwards, and at the upper and middle third of the thigh became very scarce. On the leg, foot, and lower half of arm, on the forearm and hand, the pointed form was neither so frequent nor so distinct. The pointed processes were rarely visible in the back, where, as it seemed, the disease spread equally and with the same rapidity. They became distinct in the lumbar region.

With reference to the *rapidity* of the extension in *different regions*, the author noticed that the back occupies the first place, then the scalp, shoulder, arm, and thigh. The rapidity seems greater in the direction of the points than perpendicular to them. In certain parts of the body its movement was very slow; it even passes by certain



spots entirely, and leaves them free during the whole course of the disease. Thus, for instance, erysipelas of the face extends only over the scalp to the neck and back, and thence it goes in front. The chin and upper and lower lips remain either entirely free or are only seized last. Conversely, when the erysipelas proceeds from the trunk to the head, it only occurs over the neck and the scalp, and then it attacks the ear, forehead, cheeks, eyelid, nose, and at last the lips and chin; nevertheless, the latter remains in most cases free. Further, the naso-labial fold and deep frontal folds are always spared by the disease. Erysipelas spreading from the mamma never reaches directly the abdomen, but only by the circuitous route through the skin of the back: and when it spreads from the trunk to the lower extremities, or conversely, remains stationary for days at the groin, and in the mean time proceeds over the buttocks. The skin over the trochanter margin, condyles, patella, and spine of tibia, are at first passed by, but affected two or three days later.

The author, by the comparison of his drawings and the results of Langer, touching the splitting and tension of the skin, leads to the following results:

1. There is less resistance to the spread of erysipelas in the direction of the punctated folds (*Stichfalten*) than in any other direction, on account of the peculiar direction of the tissue fascia.

2. The streaks and processes which run in the direction of the punctated folds correspond to the situations of blood- and lymph-vessels, in whose direction the exudation, with the carrier of the irritation, is borne on account of the less pressure resistance. The painfulness of the skin on pressure, which is felt in spots beyond the limits of the same, the author explains by assuming that the irritation excites at first pain for a certain distance, and then acts paralytically on the vaso-motor nerves.

3. In those parts of the body where the erysipelas halts, and which it passes by but rarely and slowly, the skin is probably of a denser structure, and attached more firmly to its substratum, for instance, at the condyles, at the iliac crests, over Poupart's ligament, tibial spines, &c. A retardation of the course occurs also when two parts of the skin, having different punctated folds in different directions, adjoin without transition.

4. That erysipelas runs both centripetally and centrifugally might be explained by the circumstance that the blood-pressure augmented by the local irritation and by the general infection expands the tissue of the skin not inconsiderably, and thus separates a little the bundles of connective tissue, and propels the exudation fluid with the bearer of the irritation to the place of least resistance, *i. e.* parallel with the split direction (*spaltrichtung*), the increased tension, and the diminished pressure.

Finally, the author mentions that also Young and Biesiadecki had found in the conium split spaces, which appeared largest in oedema of the skin. B. describes these spaces as rhomboidal, as a proof of the infectiousness of erysipelas, *i. e.* its transmissibility from one to another. Pfeleger quotes a few instances. His researches

with regard to fungus-germs in erysipelatos blood were negative in results. He further noticed that erysipelas does not always progress equally rapidly on all days, and that if on his morning visits he found a greater spread of the disease it had always been preceded by a higher temperature than on the days when the progress was less. A high temperature always preceded relapses.—*Schmidt's Jahrb.*, 1873, B. 159.

*Dr. Fiorani's New Method of Treating Orchitis.* By Dr. G. BRAMBILLA ('Gaz. Lomb.,' 38, 1872).—Dr. Brambilla contributes twenty-one cases of orchitis, treated according to the method of Dr. Fiorani, which consists in maintaining absolute rest. The patients suffered, some from blenorrrhagia, some from idiopathic, others from traumatic orchitis. In twelve cases the cause was blenorrrhagia. In sixteen cases the right testicle and in six the left was affected. The affected testis was from twice to five times the size of the normal one. The disease had lasted, on the average, a few days. The cure occupied, on the average, ten days; once it took twenty-four days. The author compares these results with the known statistics of the time occupied in the cure of orchitis, and draws the following conclusions:

1. That orchitis gets well spontaneously, much quicker and better than by the use of medicaments and artificial aid.

2. That art must assist nature by giving absolute rest to the affected organ.

*On the Treatment of Erysipelas.* By Dr. KACZOROWSKI ('Berl. Klin. Woch.,' 9, 1872).—Proceeding on the supposition that erysipelas was due to the presence of a fungus (*Micrococcus*), Dr. Kaczorowski applied the following method of treatment, which has proved successful in his hands, and which he recommends. The affected portions of the skin are gently rubbed every three hours with a mixture of carbolic acid and oil of turpentine (1 to 10), and the neighbouring parts more firmly: then these parts are covered with compresses moistened with lead lotion (1 to 100), and over these a bag containing ice, which must be frequently renewed. The patient is ordered to take lemonade or Potass. Chlor. (1 to 40), and every one or two hours a table-spoonful of sour Hungarian wine to maintain the heart's activity, and morning and evening a weak morphia injection is thrown into the neighbourhood of the affected region.

An intense redness of the skin occurs at the rubbed part, which sometimes is blistered, yet soon drying and shrinking of the epidermis occur. After twenty-four to forty-eight hours the exudation process in the skin is stopped, and is known by the fall of the skin temperature and the pulse. The author has not observed any relapses.—*Schmidt's Jahrb.*, 1873.

*Extraordinary Case of Passage of Hairs from the Bladder, giving rise to Irritation of the Bladder and frequent Micturition.* By Dr. JAMES SEWELL.—An orphan girl, æt. 3 years and 9 months, was admitted on the 18th May, 1873. Her intellect is weak and speech backward, and her habits very dirty. It was noticed for some time



before admission, by the family which had adopted her, that she made water very frequently, and suffered more or less pain. On examining the urine a number of hairs, varying from one to seven inches in length, were found.

On microscopic examination each hair showed a well-defined bulb, and all the characters of ordinary hair. At the time of writing, Dr. S. said that she was still suffering from vesical irritation, and passed about a dozen hairs daily. Generally they were fine, and of a light colour, but occasionally they were coarse and dark. The case, although uncommon, is not unique, as Coulson reports three cases from the practice of Sir Henry Sloane, and one in that of another gentleman, and some cases have since been reported. One feature is peculiar to the case, viz., that the child is constantly drawing similar hairs, from eight to ten inches long, from her mouth. These are frequently coated with bloody mucus. No calcareous matter is attached to the hair found in the urine, which is of sp. gr. 1018, and contains neither blood, pus, nor mucus. The hairs were found in the urine that was passed in Dr. Sewell's presence.

*Papilloma of the Umbilicus.*—The 'Nuova Leguria Medica,' January, 1873, reports a case of removal, by Dr. Rizzoli, of a large and deep papilloma of the umbilicus. Papillomata and can-croids of the umbilicus are very rare, and their removal by the knife has generally ended fatally. Rizzoli mentions several fatal cases occurring in his own practice.

The patient was a female, æt. 51, and the tumour did not extend beyond the ring. The surface of the tumour was covered with a paste of chloride of zinc eight grammes dissolved in alcohol and mixed with eight grammes of flour and charpie and a bandage applied. The pain and inflammation were not very severe, and after nine days the papilloma was reduced to an eschar, which became detached, leaving a clean conical wound, with its base external. The pointed part corresponded precisely with the umbilical ring, which remained intact. The wound healed nicely, leaving a depressed cicatrix.

The 'New York Medical Journal' also reports a *remarkable case of fibroid tumour*, which was removed from the left labium of a child aged  $2\frac{1}{2}$  years. It measured five inches in length and three in breadth. The little patient recovered rapidly from the operation.

*Compression of the Facial Artery for Epistaxis* ('Gaz. Med. Lomb.,' 1873, and 'Rev. de Thérap.').—Dr. Marvin, of Geneva, disapproving of the disagreeable process of plugging the noses with either a Belloc's sound or an elastic catheter, recommends the following method:—As the blood generally comes from one side of the nose only, and most frequently from the anterior third of one of the nasal fossæ, he merely compressed the corresponding facial artery against the superior maxilla, near the angle of the nose. The afflux of the blood to the nasal cavity is thus diminished, and the epistaxis ceases almost instantly.

Bessières, in 'La Frana Medica,' 1873, recommends plaster of Paris for arresting epistaxis. The plaster is known as a hæmostatic

in cases of leech-bites, cuts, excoriations, and after tooth extraction. The method of using it in epistaxis is as follows :—Sift a spoonful of unslaked plaster through a coarse sieve, place it in a tube of paper, and blow it forcibly into the nostril, after having made the patient blow his nose.

*Plastic Surgery of Depressed Nipple* ('Centralblatt,' April, 1873).—In the above-named journal Dr. Kehrer proposes a plastic operation—excision of the areola—in those cases in which the nipple is depressed to such a degree as to prevent every attempt at aspiration and the margins of the crater-like depression only approximate by the use of mechanical means. By this operation the nipple is rendered level, and an artificial suction made prominent. It consists in a circumcision of the areola, an incision above being made to meet one from below at an acute angle at the middle of the lateral margin of the areola. The upper incision corresponds to the margin of the areola. The skin and smooth muscular fibres are then carefully dissected in the direction toward the nipple, care being taken not to injure the lacteals until the nipple is completely isolated. A portion of the flap,  $\frac{1}{4}$  mm. in width, is then cut off, the hæmorrhage checked by cold, and the edges of the wound united. After union has taken place, the nipple is on a level with the surrounding tissue, or projects slightly beyond.

The use of suction-glasses is then indicated, care being taken to prevent vesication through over use. This operation has been performed on two patients, and in one case on both breasts.

*Neuralgia of the Testis* ('Wien. Med. Presse,' 1872).—Dr. Lazarus specifies, among the less known causes of this affection, chronic disorders of digestion, and long absence from sexual intercourse. The former are often accompanied by pain in the testicles, and in the latter the pain seems to be due to a temporary debility of the virile powers.

Dr. Lazarus has had great success in many obstinate cases by the internal administration of Zinci Sulph. (0·2 to 200) three times a day, a table-spoonful, and has also hypodermically injected behind the scrotum a solution of the same salt. The general health must be attended to.

*The Treatment of Hæmorrhoids by Linear Cauterization*. 'La France Méd.,' No. 96, 1873.—Dr. Voillmier prefers linear cauterization for the radical cure of piles to any other surgical treatment. He has thus treated forty-three patients, and has obtained, in all cases, a cure without any accident. The operation is easy, quick, and does not cause much pain, because of its rapidity. It deserves, then, to be tried—for it is not a new operation—in preference to the ligature excision, crushing, &c., which are liable to dangerous complications. The patient, whose rectum is empty, is placed in a convenient position at the edge of the bed, and anæsthetized if he desire it. The anus and neighbouring parts are well painted with collodion, and evaporation hastened by blowing, without which it may cause inflammation. The cautery should be two centimètres long and 1 thick, having an extremity and a cutting edge, and the



back sufficiently thick to retain the heat. The surgeon introduces the cautery into the anus to the depth of a centimètre, and applies the heel of the instrument to the cutaneous orifice a little more than to the mucus, and cauterises in four directions—anteriorly, posteriorly, and to the right and left. The operation lasts only a few seconds.

The only application consists of compresses dipped in cold water. Should swelling come on, or the hæmorrhoidal tumour appear to return under the influence of the congestion occasioned by the cautery, there is no need to heed it. If there be pain a narcotic plaster and a poultice should be applied.

This method has been practised for some time in the London hospitals; but instead of using a cutting cautery a platinum wire is heated, and allowed gently to eat its way through the base of the pile. The hæmorrhage which accompanies rapid excision by the cautery is thus prevented.

*Remarks on Varicocele and Varices of the Lower Extremities.* By Dr. NATHAN BOZEMAN ('New York Med. Journ.,' No. 4, October, 1873).—In an excellent paper on this subject, accompanied by cases and new operative indications and methods accompanied with illustrations, Dr. Bozeman draws the following conclusions:

1. That varicocele has its commencement in boyhood, though frequently not noticed till after the age of twenty-one, and that it is usually the result of self-abuse.

2. That the old theory of the greater length of the spermatic vein, and its peculiar mode of entering the corresponding renal, does not fully explain the cause of the affection.

3. That obliteration of the spermatic veins does not always result in a cure of the disease and removal of its effects upon the system.

4. That retrenchment of the scrotum, in connection with obliteration of the spermatic veins, constitutes a most important part of any system of treatment, and in mild cases is sufficient alone to effect a cure.

5. That for obliteration of the spermatic veins the procedure described in the paper is the simplest, safest, and most efficient, that can be employed. (The difference between Dr. Bozeman's method and that recommended by Professor Gross consists in the use of silver wire, and the button-shield fastened with a perforated shot.

6. That varices of the lower extremities in the milder forms are most frequently met with among women; but, in the worst forms with associate ulcers upon the legs, they are oftener seen among men, owing to the heaviness of their labour and the greater persistence of their muscular efforts.

7. That varices with associate ulcers upon the legs are generally found between the ages of thirty and fifty, and that in old subjects, with broken constitutions, canalization of the veins is often seen.

8. That varices and varicose ulcers of the legs stand in the

relationship of cause and effect, and that the permanent cure of the latter can only be effected by the obliteration of the former.

9. That varices of the external saphena vein is seldom met with in its usual course to the popliteal, and an ulcer in its track is thought to be still rarer in its occurrence; but when it joins the internal saphena in its anomalous course it becomes much more liable to disease, with ulcers on the outside of the ankle.

10. That varices in the worst forms are perfectly curable with the silver wire ligature, used in the form above described.

11. That for the operation the patient should be required to stand, as in this way the prominence of the veins is maintained, and greater facility given to the passage of the needle.

12. That when the operation is properly performed it is perfectly simple, and little liable to the dangers usually feared and described by authors.

13. That the transfixion of the vein with the needle and the lodgment of the silver wire there eight days are not liable to be followed by any untoward symptoms.

14. That the wire does not effect complete division of the vein under the constricting force applied to it, nor is it necessary that this, or even a partial division, should take place in order to ensure obliteration.

15. That eight days usually suffice for the apparatus to remain in position, when it is usually removed by clipping off the shot and withdrawing the wire.

16. That ulceration of the skin and exposure of the vein, the main causes of accidents arising from the operation, are effectually guarded against.

17. That obliteration of the vein at the ligated points is due usually to agglutination of the sides of the vein, the result of adhesive inflammation, previously induced in the endothelial membrane from pressure of the constricting wire.

18. That the method is applicable to all cases, and the cures in a very large proportion remain permanent, even in the worst forms of the disease.

*Primary Croup of the Nasal Mucous Membrane* ('Jahrb. für Kinderk.,' p. 314).—Dr. Schuller communicates the following case, which belongs to a class of uncommon affections, and which had an unusual course.

A boy, æt. 5, of healthy parents, but who had suffered from catarrh from his birth, was affected with dyspnœa and convulsive attacks. He had been nursed by a wet-nurse. A floating croupous membrane was discovered in the left nasal cavity; no fever and no cause for diphtheria or syphilitic coryza to be found. The croupous membrane was removed, but returned in two days, and after this also had been removed a copious hæmorrhage followed from the nose. Simultaneously there appeared, on the right under lid, a red spot the size of a hazel-nut, which disappeared the next day, when another appeared, with feverish symptoms, in the vicinity of the malar bone, which, during the following days, extended in all direc-



tions over the surface of the skin, with symptoms of erysipelas or erythema. The croupous process also extended through the nose to the throat. The child died in a convulsive attack.—*New York Med. Journ.*, vol. xvi, p. 74.

*A New Operation for Anchylosis of the Elbow-joint resulting from Fracture, and Rigidity the result of Unreduced Dislocation.*—Dr. P. H. Watson, of Edinburgh, in a recent pamphlet, with which he has favoured us, describes an operation, which he has devised and practised with success for the removal of the elbow-joint in a class of cases which are unfortunately sufficiently common, more especially in the surgical practice of the larger hospitals.

After stating that excision of the elbow has long been recognised as a legitimate procedure in cases of injury or disease of the articulation resulting in partial or complete anchylosis, because the movements of the limb are much more perfect after the operation, he says that, in his experience, the limb resulting in cases of excision for anchylosis has not been so satisfactory as when the joint has been excised for active disease. “On the one hand, too great a degree of mobility in every direction has been the result; on the other, the union between the divided ends of the bones has been more complete than could be desired, and the movements have been commensurately imperfect.”

In one case he practised resection no less than three times, removing freely the osseous textures, but on each occasion ossific growth rendered the movements of the limb very unsatisfactory.

When too great movement has been the fault, a want of muscular development was apparently the chief obstacle to the usefulness of the limb, and the loss of the action of the triceps in extension was the main deficiency. Reflecting upon these drawbacks, Dr. Watson practised his first modification of the operation in the summer of 1869. This consisted in an attempt to save the insertion of the triceps in a case of fracture of the ulnar end of the humerus by subperiosteal resection. He made a straight incision through the triceps and over the olecranon; the insertion of the triceps and the periosteal covering of the ulna were turned aside, and the articular surfaces removed. The result was not encouraging, extensive osseous deposit impeding the movements. He repeated this method upon two other occasions, but in both there was too little movement to satisfy every requirement.

In the summer of 1871 Dr. Watson operated on a boy by a new method, which he conceived would fulfil every indication. The speculative reasonings which led him to adopt it were these:—It was quite obvious that in most cases of fracture into the elbow-joint the humerus was the bone alone affected; no changes in the osseous structures of the radius and ulna necessarily resulted from any injury the humerus had sustained; nor even should the radius and ulna be involved in the injury, did the resulting efforts at repair constitute a condition which implied any need for their removal by operation. It was also obvious that the removal of the upper extremity of the ulna necessarily impaired the perfection of the mus-

cular attachments, viz. of the triceps and the brachialis anticus, and indirectly the power of the triceps in flexion of the forearm. It was clearly, therefore, very desirable that neither the radius nor ulna should be interfered with if removal of the extremity of the humerus alone would suffice to remedy the ankylosis.

The operation is thus described:—1. A linear incision to the inner side of the olecranon over the ulnar nerve rather longer than that usually employed. 2. The ulnar nerve is then turned over the internal condyle by careful dissection. 3. A probe-pointed bistoury is introduced into the joint in front of the humerus, and then behind that bone, and carried upwards, so as to divide the capsular attachments in front and behind. 4. The entire internal condyle and trochlea to be removed by bone-forceps, which are then introduced in the opposite diagonal direction, so as to detach the external condyle and capitulum. 5. The truncated and angular end of the humerus to be cleared, turned out through the incision, and smoothed across at right angles to the line of the shaft by means of the saw, whereby (6) room might be afforded, so that by partly twisting, partly by dissection, the external condyle and capitulum might be removed without any division of the cutaneous tissues on the outer side of the arm. This internal incision permits of the discharges being thoroughly drained, and allows the extremity of the humerus to be removed entire, and without interfering with any muscular structures, except those of the forearm, which take origin from the osseous structures actually excised.

Dr. Watson operated thus six times, and all the cases except one were satisfactory. In this single case an attack of osseo-myelitis supervened and osseous union was threatened, but the secondary removal of a further slice of the humerus afforded an ultimately satisfactory issue.

Dr. Watson recommends this operation, which is original with him, on the following grounds—1. It leaves the attachments of the triceps and brachialis anticus undisturbed. 2. It limits the area of operation almost exclusively to within the capsule of the joint, which seems to secure more speedy healing of the wound. 3. There is less ultimate surface deformity, a more direct drain for discharge, and a more ready access to the ulnar nerve.

Only one objection can be taken to this method of procedure, viz. that it does not afford a ready access to the external lateral ligament; this, however, is of trivial importance if the plan of operating he has laid down be rigorously adhered to in the division and removal of the end of the humerus, viz.—1. The oblique division of the humeral condyles between the trochlea and capitulum from above downwards. 2. To cut off the capitulum and external condyle obliquely from the shaft by means of pliers applied from below upwards. 3. To turn out and remove as much of the truncated and conical extremity of the shaft as deemed requisite; and, lastly, to dissect and twist away the capitulum and external condyle from their remaining ligamentous and other attachments.

This operation would, of course, be more difficult in cases of true



bony ankylosis; but this condition is uncommon, and may be simulated by the altered form of the osseous surfaces resulting from the fracture and displacement, and at most the ankylosis is usually fibrous in its character.

Should the ankylosis prove to be bony, forcible flexion and extension under chloroform will, in the great majority of such cases, effect such a degree of solution of continuity as will enable the operation to be carried out without great difficulty in the manner described; but if there be very dense bony union, rendering the risk of fracture of the olecranon or of the shaft of the humerus a reasonable danger to be avoided, then a transverse section of the humerus through the condyles, excision of a portion of bone above this level, and piecemeal excision of the ankylosed condyles themselves by the forceps and gouge, would afford an alternative means calculated to remove any ordinary difficulties, while the conversion of the operation into a complete excision of the elbow may always be had recourse to should insuperable obstacles be found to prevent the execution of the more limited excision.

Injuries to the elbow-joint are very common, especially in children, according to our experience, and are unfortunately too often followed by more or less impairment of motion, and we have long been in the habit of preparing the friends to expect immobility in varying degrees. If the injury, whether a sprain, or fracture, or dislocation, end in ankylosis at an inconvenient angle, we should first try to remedy the malposition by manipulation under chloroform, and afterwards keep up passive motion; but should these measures prove unavailing we think that Dr. Watson's operation would not only be justifiable, but should be resorted to, backed up as it is by several successful cases. It must be recollected that Dr. Watson does not intend this method in cases of *disease* of the articulation, but only in cases of ankylosis, whether due to injury or disease.

REPORT ON TOXICOLOGY, FORENSIC MEDICINE,  
AND HYGIENE.

By BENJAMIN W. RICHARDSON, M.D., F.R.S.

## I. TOXICOLOGY.

*On the Toxicological Properties of Putrefying Blood.*—We select, as the first contribution for notice this time, a paper by Drs. Gesualdo Clementi and George Thin. These savants have conducted, in the Institute for Experimental Pathology in Vienna, a series of researches on the toxicological qualities of decomposing blood, which extend our knowledge on the subject in hand more considerably than any other communication that has been published during the past two years. To all our readers the researches and results of these authors will be most valuable, as bearing on the whole question of contagion from animal poisons; we venture, therefore, to notice them with more than usual detail, and with one or two preliminary historical notes.

Majendie first discovered that decomposing blood introduced into the veins or into serous cavities of the living animal induces death, but his experiments, like most of those which are first instituted on any scientific subject, were not sufficiently minute to attract instant attention, and were, indeed, allowed to pass for many years little heeded. Majendie also, according to Davaine, recognised that diseases could be transmitted through organic fluids rendered poisonous by inoculation of infected matter, and that the virulence of the induced affection increased by transmission of the poison.

In 1865 we ourselves conducted an inquiry on this same subject, with a sanguineous fluid that had been removed from the peritoneal cavity of a patient on whom Mr. Spencer Wells had performed ovariectomy. The fluid was drawn off a short time before the death of the patient, and although it seemed free of putrefaction it proved to be extremely poisonous. We found that inoculation from it by the hypodermic needle caused elevation of temperature in the rabbit, and death within four days, with symptoms similar to those in the human subject from whom the poison was taken. We discovered that some fluid matter taken from the abdominal cavity of the infected animals produced the same disease in other similar animals, and finally we produced from a portion of the original fluid a chloride salt, which also infected, from which circumstance we were led to infer that the poisonous product was alkaloidal in character.

The results thus arrived at,—the report of which formed the substance of a paper to the British Medical Association, at its meeting in Leamington, in 1865, and which were also related, in 1866, in a lecture delivered at the same town, “On the Poisons of the Spreading Diseases,”—led us to give the name of “septine” to the organic



poisonous product; a product we have endeavoured to isolate, but hitherto without success.

Messrs. Clementi and Thin state in their essay that in 1866 MM. Coze and Feltz conducted their experiments, and in 1869 Davaine began to experiment with putrefying ox's blood, and determined, (*a*) that the blood was poisonous when inoculated into the bodies of rabbits and guinea-pigs; (*b*) that blood from the animals that died after inoculation produced, before it putrefied, the same disease in other animals inoculated with it; (*c*) that blood of the next and of succeeding generations of inoculated animals conveyed still the poisonous quality; and (*d*) that the fatal quantity required was reduced in amount as the transmission was advanced. Thus progressing, Davaine obtained at last, as he believed, poisonous effects from doses of poison which may be called, truly, infinitesimal. After Davaine, Beheir, Vulpian, Bouley, and Onimus, pursued, by modified processes, the same path of inquiry, and, later still, the authors whose paper lies before us commenced and carried out their remarkable researches.

Messrs. Clementi and Thin conducted their research at the Institute for Experimental Pathology at Vienna, the resources of which institution was put at their disposal by Professor Stricker. Their experiments were commenced in the end of February, 1873, and were closed in the beginning of May. The number of animals on which experiments were made by injecting healthy, putrefied, and transmitted blood, amounted to 123 rabbits, 6 guinea-pigs, and two dogs. The injections, say the authors, were invariably made into the subcutaneous cellular tissue of the neck. The instruments used were those which are ordinarily employed for subcutaneous injection.

The dilutions were made after the manner of the homœopathists. The capacity of the syringe used was 0·80 cubic centimètre. It was marked with decimal divisions, and one of these divisions was taken as unity in the dilutions. In all doses which did not exceed 0·08 gramme, the mass of injected fluid was 0·08. The dose 0·08, the smallest they could accurately measure with the syringe, represented the corresponding quantity of undiluted blood; 0·008 represented blood mixed with ten times, 0·0008 with a hundred times, its volume of water. The blood was sometimes taken from the animals when they were mortally sick, and sometimes after they had died from inoculation. As the observers did not occupy themselves specially with the symptoms of the disease produced, no pretension is made to give an accurate and exhaustive history of it, further than to state briefly that the animals gave up eating, stood motionless in their stalls, were affected with dyspnœa, leant to one side in a very characteristic manner, and towards the end were frequently convulsed, especially if they were moved. The swelling of the muzzle which was sometimes present was inconstant. The blood of the affected animals was repeatedly examined under the microscope at different stages of the malady. The specimen examined was usually obtained by inserting a fine glass tube into the jugular vein. Rounded or irregularly-shaped granular elements, varying in size, were present in great

number; the white corpuscles were strikingly large, and were present in a greater proportion than in normal blood. The blood frequently coagulated under the microscope with great rapidity, so that the known plâques composed of threads and granules were visible. Vibrios and bacteriæ were not once seen in blood taken from the living animal, even when it was *in articulo mortis*; whilst they were found abundant in every specimen of putrefied blood put under the microscope.

In the following tables the observations of the authors are epitomised :

TABLE I., giving a general view of the Experiments on Rabbits.

| Kinds of blood injected. |                        |  |  | Num-<br>bers. | Died. | Sur-<br>vived. | Total. | Died. | Sur-<br>vived. |
|--------------------------|------------------------|--|--|---------------|-------|----------------|--------|-------|----------------|
| Healthy {                | Dialyzed . . . .       |  |  | 2             | 0     | 2              | 27     | 4     | 23             |
|                          | Pure . . . . .         |  |  | 25            | 4     | 21             |        |       |                |
| Putrefied . . . . .      |                        |  |  | 23            | 11    | 12             | 23     | 11    | 12             |
| Trans-<br>mitted {       | Pure . . . . .         |  |  | 49            | 37    | 12             | 73     | 54    | 19             |
|                          | Boiled . . . . .       |  |  | 11            | 9     | 2              |        |       |                |
|                          | Dialyzed . . . . .     |  |  | 8             | 8     | 0              |        |       |                |
|                          | Treated with alcohol . |  |  | 3             | 0     | 3              |        |       |                |
|                          | Distilled . . . . .    |  |  | 2             | 0     | 2              |        |       |                |
|                          |                        |  |  |               |       |                | 123    | 69    | 54             |

TABLE II.—Inoculation with the Blood of Healthy Animals.

| Dose in<br>cubic centi-<br>metres. | Number of<br>animals<br>inoculated. | Died after inocula-<br>tion.  | Survived. |
|------------------------------------|-------------------------------------|---|-----------|
| 0·4                                | 4                                   | 0   | 4         |
| 0·16                               | 8                                   | On the 5th day<br>On the 3rd day<br>On the 9th day<br>On the 20th day | 4         |
| 0·08                               | 13                                  | 0   | 13        |
|                                    | 25                                  | 4   | 21        |



Table I shows that the mortality amongst the animals inoculated with transmitted blood was at the rate of 73·9 ; with putrefied blood, 47·8 ; and with healthy blood, 15 per cent.

TABLE III.—*Inoculation with Putrefied Blood.*

| Number.                                       | Number of Animals.                          | Dose in cubic centimetres.                    | Died after inoculation.         |   | Survived.   |                                 |   |
|---|---|---|---------------------------------|---|---|---------------------------------|---|
|   |   |   | Number.                         |   | Number.   |                                 |   |
| 1 }<br>2 }<br>3 }<br>4 }<br>5 }<br>6 }<br>7 } | ...<br>...<br>...<br>7<br>...<br>...<br>... | ...<br>...<br>...<br>0·4<br>...<br>...<br>... | {<br>{<br>{<br>{<br>{<br>{<br>{ | ...<br>...<br>...<br>5<br>...<br>...<br>... | On 2nd day.<br>2nd "<br>2nd "<br>2nd "<br>3rd "<br>...<br>... | {<br>{<br>{<br>{<br>{<br>{<br>{ | ...<br>...<br>...<br>2<br>...<br>...<br>... |
| 8 }<br>9 }<br>10 }<br>11 }                    | 1<br>...<br>3<br>...                        | 0·24<br>...<br>0·16<br>...                    | {<br>{<br>{<br>{                | 1<br>...<br>1<br>...                        | ,, 11th "<br>,, 7th "<br>...<br>...                           | {<br>{<br>{<br>{                | ...<br>...<br>2<br>...                      |
| 12 }<br>13 }<br>14 }<br>15 }<br>16 }          | 1<br>...<br>4<br>...<br>...                 | 0·08<br>...<br>0·00008<br>...<br>...          | {<br>{<br>{<br>{<br>{           | ...<br>...<br>2<br>...<br>...               | ,, 2nd "<br>...<br>,, 12th "<br>...<br>...                    | {<br>{<br>{<br>{<br>{           | 1<br>...<br>2<br>...<br>1                   |
| 17 }<br>18 }<br>19 }<br>20 }<br>21 }          | 1<br>...<br>4<br>...<br>...                 | 0·000008<br>...<br>0·0000008<br>...<br>...    | {<br>{<br>{<br>{<br>{           | ...<br>...<br>1<br>...<br>...               | ,, 9th "<br>,, 12th "<br>...<br>,, 3rd "                      | {<br>{<br>{<br>{<br>{           | ...<br>...<br>3<br>...<br>...               |
| 22 }<br>23 }                                  | 2<br>...                                    | 0·00000008<br>...                             | {<br>{                          | 2<br>...                                    | ...<br>...  | {<br>{                          | ...<br>...                                  |
|   |   |   | 12                              |   |   | 11                              |   |

If Table III be compared with Table II a striking difference is observable in the results of inoculation with the larger doses. Of 7 animals that received 0·4 cub. cent. of putrefied blood, 5 died ; while of 4 that received a like dose of healthy blood, all survived. It is to be remarked that the two animals which had received doses of 0·00000008 cub. cent. of putrefied blood died, whilst the mortality from larger doses was much smaller. In the animals that died after inoculation with transmitted blood extensive infiltration of the skin and subcutaneous cellular tissue was found at the part where the injection had been made.

In another table describing the effects of inoculation with transmitted blood, it is shown that of 29 animals that received doses of 0·08 to 0·00008 cubic centimetres, only one survived, and that this single exception belonged to the second generation ; that only one lived to the 10th, one to the 6th, and four to the 5th day after inoculation.

That the deaths were not from other causes was proved by the

fact that the part where the inoculation was effected was, without exception, the seat of infiltration.

The dose decreases from 0·000008 cub. cent., and the rate of mortality begins then to decrease in a striking manner; but the experiments were not sufficiently numerous to define the degree of dilution where the poison ceases to take effect. The mortality in the animals inoculated with transmitted blood was very much greater than in those inoculated with putrefied blood; an approximate dose of 1 decimilligramme of transmitted blood was sufficient to kill a rabbit. When it is considered that in these experiments transmitted blood to the twelfth generation was used, it becomes evident, the authors believe, that the noxious agent must have multiplied in the animals after inoculation. For even if it be supposed that the blood alone, and not the other fluids of the animal's body, acted as a diluent of the dose injected, there would be, by the time of the twelfth generation, fractions of a gramme in a dose that would have to be written with 50 to 60 ciphers. But the experiments made with doses written with ten ciphers gave already negative results.

As, in accordance with accepted definitions, the noxious substances present in putrefied blood must be designated as miasmata, and those conveyed from one animal to another and multiplied in the organism of the animal as contagia, the authors conclude that a miasm has become transformed into a contagium. As to the nature of the miasma and the contagium they express no opinion, their experiments being defective in this direction. But they made the following experiments with the view of narrowing the question.

To test the experiments of Onimus, who maintained that the poison is not dialyzable, they constructed a dialysis apparatus with parchment. This apparatus, containing blood from animals dead and sick from inoculation with transmitted blood, was allowed to rest in water for twenty-four hours. The experiments then made show that the inference that the poison is not dialyzable has no foundation.

They also boiled and filtered blood from the sick animals, and injected both the filtrate and the coagulum broken up in water.

The surprising fact was ascertained by these experiments, that the poison is not destroyed by boiling.

The blood of affected animals was mixed with alcohol, and allowed to stand twenty-four hours, and the coagulum washed with water and injected. Three animals so treated all survived.

Lastly, they distilled the infected blood, and injected the product of distillation into two animals. Both of these survived.

The most important result of the post-mortem examinations was, the infiltration of the cellular tissue of the neck. Microscopic examination of the infiltrated fluid showed it to be full of pus-corpuscles, and an immense number of small granular elements, partly similar to those found along with torn pieces of white blood-corpuscles in the affected blood.

In a number of cases the experimenters found considerable swelling of Peyer's plaques, which in one case were ulcerated.



In two cases they found pleuritic exudation. Pericarditis was observed several times. But none of these lesions were constant, the thoracic and abdominal organs being often found perfectly normal.

From a few experiments made on guinea-pigs the authors learned that blood taken from a guinea-pig that was suffering from the injection of putrid blood, produced death in two others at the dose of half a drop diluted in water, and this although the guinea-pig that furnished the poisonous blood recovered. They met with an analogous fact in the rabbit.

The following experiments in connexion with those made in Paris with the blood of typhoid fever are added.

Blood was taken from the finger of a man in the General Hospital at Vienna, who was in the twelfth day of typhoid fever, from which he died two days afterwards. It was diluted with distilled water, and injected into two rabbits. One received an approximate dose of 0·008 cub. cent., and died twenty-eight days afterwards. The other received an approximate dose of 0·08 cub. cent., and died twenty-two days afterwards. In both these rabbits putrefaction set in after death with unusual rapidity. To control the experiment, the blood of the rabbit which received the dose of 0·08 cub. cent., was injected into two others at the dose of 0·0008 cub. cent. One of these died eleven days afterwards.

Blood was taken from the finger of a girl ill of scarlet fever in the Children's Hospital in Vienna, and in the period of full eruption of the disease. The child died the following day. The blood was diluted with distilled water, and injected into two rabbits. One received an approximate dose of 0·0008 cub. cent., and thirty-eight days afterwards was unaffected. The other received an approximate dose of 0·008 cub. cent., and died twenty-one days afterwards.

Blood was taken from the finger of a woman ill of variola hæmorrhagica, in the eighth day of the disease, diluted with distilled water, and injected into two rabbits. One received an approximate dose of 0·00008 cub. cent., and died nine days afterwards. The other received an approximate dose of 0·008 cub. cent., and died fifteen days afterwards. From the latter blood was taken and injected into another rabbit, in a dose of 0·0008 cub. cent. This rabbit died in two days.—*Pamphlet reprinted from the 'Edinburgh Medical Journal,' 1873.*

*Toxicological and Physiological Action of Emetine.*—A. Evaristo d'Orenellas has made a new and important study of the action of emetine. The fatty, odorous, nauseous principle of ipecacuanha is only an accessory in the action of the drug; the emetine is the active part. Emetine, when applied topically, acts as an irritant to the mucous membrane: it also irritates the skin in parts from which the epidermis has been removed; it does not irritate the sound skin. When injected into the cellular tissue it induces vomiting in man and in other animals which are subject to this symptom,

but not so determinately as when it is taken into the stomach. Once introduced into the circulation, it is eliminated entirely by the gastro-intestinal tract and by the liver. It is eliminated also with the vomited matter which it excites the stomach to throw off.

The physiological mode by which the agent causes vomiting is reflex; it is from irritation communicated to the peripheral termination of the pneumogastric in the stomach and duodenum. Its action, therefore, may be considered topical. In moderate doses it reduces the respiration and circulation; it also reduces the animal temperature, but it does not directly modify the vascular tension. In larger doses, such as produce vomiting, the action is the same, but more energetic. If the action be continued many days, even with small doses, hepatization of the lung from continued congestion is a source of danger; but the circulation is less rapidly decreased than the respiration. On the animal temperature the action is peculiar in that the superficial cutaneous temperature is decreased, while the internal temperature—that of the rectum, for example—is increased. On the muscular system the substance acts so as to cause relaxation, and it combats the convulsive phenomena induced by strychnia.—*Rivista di Medicina, Chirurgia e Terapeutica*, and *Annali di Chemica*, April, 1874.

*On the Poison of Naja tripudians and other Indian Venomous Snakes.*—Drs. Brunton and Fayrer, from a most laborious research on the subject named above, have arrived at several noteworthy facts. We refer to one or two of these. The authors show that in most cases of death from cobra poison the fatal issue is not to be attributed to any failure of the circulatory apparatus, for the heart continues to pulsate vigorously long after all motions have ceased in the voluntary muscles, and after the strongest irritation applied to the spinal cord and motor nerves fails to produce the slightest effect: but this only occurs when the dose of the poison is not excessive; for when a large quantity of it is introduced at once into the circulation the heart is not exempted from its action, but is, on the contrary, most seriously affected. The action of the poison upon the respiration is, perhaps, after all, the most important of those which it exerts upon the organism, for it is through this action that death is generally caused. The respiratory movements, besides being altered in form, are generally quickened after the introduction of the poison; then the number sinks to the normal, or even below; they become weaker, and finally sink altogether. The blood being no longer aerated becomes more and more venous, and by irritating either the respiratory centre itself or some nervous centre closely associated with it, occasions general convulsions. These disappear whenever artificial respiration is set up, and the blood is again aerated. The authors are of opinion that the poison is excreted by the kidneys and mammary glands, and probably also by the salivary glands and mucous membrane of the stomach.

On the subjects of antidotes for the cobra poison the authors speak with great doubt. The injection of ammonia into the veins



as a means of cure does not, according to their experience, yield any useful results. They draw this inference from experiments they themselves have conducted on the inferior animals, and they support it by reference to two cases of poisoning in the human subject, in which Dr. Hilson injected ammonia into the veins, but without producing any good result. Both patients died. To prevent absorption of the poison the authors suggest the speedy application of an elastic band round the limb, such as is used in bloodless operations, combined with the application of cups attached to an exhausting pump.—*Proceedings of the Royal Society*, January, 1874.

## II.—FORENSIC MEDICINE.

*Examination of blood-stains.*—MM. Mialhe, Mayet, Lefort, and Cornil, point out that among the fluids which are destructive of blood-corpuscles are water, particularly when hot, acetic gallic, sulphuric and hydrochloric acids, potash and soda even in weak solutions, ether, chloroform and other reagents. On the other hand, alcohol, chromic and picric acids, and potassium dichromate, preserve the corpuscles, though they alter their form. The preservative liquids are those whose composition approaches nearest to that of serum, such as the iodized serum of Schultze, an excellent preparation made with amniotic fluid to which are added a few drops of the tincture of iodine, so as to give it the colour of white wine, or, better, a liquid thus composed:—White of egg 30 grams, distilled water 270 grams, sodium chloride 40 grams, or even a liquid containing 0·5 per cent. of sodium chloride, or 5 or 6 per cent. of sodium sulphate. If the stains be wetted and softened by these liquids and then examined, white and red corpuscles and fibroid particles will be observed.

In more difficult cases, when the microscope can give but vague information, owing to the alterations which time has effected in the hæmatin, examination by the spectroscope and chemical analysis are necessary to arrive at precise results. If into an analysing tube nearly filled with water a few drops of blood be introduced till the liquid has the colour of peach blossom, the luminous rays of the spectrum passing through this fluid present two bands of absorption between the lines D and E of Fraunhofer, in the yellow and green. In a case of doubt the hæmoglobin of the blood could be reduced by adding a reducing body to the latter. Destroyed hæmoglobin has a different spectrum from oxygenated hæmoglobin—a single absorption band as large as the two former bands united, and a little to the left of Fraunhofer's line D.

In blood in a state of decomposition, or which has been treated by acids or alkalies, hæmoglobin is changed into a new substance; hæmatin is formed, which gives characteristic crystals when combined with hydrochloric acid. To obtain these crystals a small fragment of dried blood is dissolved in a drop of water on a glass slide, and a minute portion of sea-salt is then added. It is then covered with a thin slide, pure acetic acid is made to pass between the two slides,

and it is heated over a spirit lamp to the boiling-point. Acetic acid is again added, and it is heated afresh, and this treatment is repeated until the crystals are obtained. They are rhomboidal, of a dirty brown colour, quite characteristic, and require a magnifying power of three or four hundred diameters. With the smallest quantity of blood these two reactions can always be obtained—the spectrum examination and the crystals of hæmatin hydrochloride, and they are so certain that the existence of one alone may be regarded as decisive of the presence of blood.

A third process, though not so exact as the preceding, ought not to be neglected. If to a very small quantity of blood dissolved in a little water there be added a few drops of tincture of guaiacum and of hydrogen dioxide, a persistent blue colour is produced; but this very sensitive reaction can be obtained with other organic matters—nasal mucus, saliva, &c. A tincture of guaiacum is prepared with alcohol of 83° and guaiacum resin; a mixture of ether and hydrogen dioxide is also made, and kept under water in the dark. This latter preparation is less liable to change than pure oxygenated water. The object stained with blood, if it be white, is put into a little cup, then moistened with water to dissolve out the blood-stain, and washed in distilled water. This water is then submitted to the action of the above reagents. If the fabric stained be coloured, and the stain be little or not at all visible, it must be moistened, and then pressed between two or three sheets of white blotting-paper, and tried first with the guaiacum. If the stain be of blood, a reddish or brown spot will form on the paper. One of the sheets should be treated with ammonia, and the stain will become crimson or green. A second sheet treated with tincture of guaiacum or ozonized ether will give a blue colour, more or less intense, according to the quantity of the blood.—*J. B., in Journal of the Chemical Society, April, 1874.*

*Signs of Death from Suffocation.*—Dr. David Page opposes the statement of Tardieu, that the presence of subpleural ecchymoses in cases of hanging constitutes clear proof of criminal violence; and that if seen on the lungs of an individual found hanged murder has been accomplished, in the first place by suffocation, and the body has afterwards been suspended in order to simulate suicide. In order to ascertain the truth of his views Dr. Page performed a series of experiments in which animals were subjected to death by various kinds of arrested breathing—by suffocation in cinders, by strangulation, by hanging, by drowning. From these researches he comes to the following conclusions:

1. The ecchymoses, or patches of extravasated blood, found on the surfaces of certain of the internal organs, and notably of the lungs, are not peculiar to any one mode of death from apnoea, but are common to all.

2. The ecchymoses are not diagnostic of suffocation, as has been maintained by M. Tardieu.

3. These lesions probably occur with greater frequency in suffo-



cation, owing to the absence of interference with the cerebral circulation, and the opportunity which the means usually employed afford for the respiratory struggles.

4. The value of these lesions, from a medical point of view, must be determined by the conjunction of other signs of apnœa, and the production of proof that they are not the spontaneous results of disease.

It is not the isolated fact, the so-called pathognomonic sign, which is to be always looked for, but that concurrence of facts or signs which, of no specific value if taken one by one, afford then incontestable proof by reason of their mathematical integrity.—*Prize Thesis of the University of Edinburgh*, Blackwood and Sons, 1873.

### III.—HYGIENE.

*Transmission of Syphilitic Contagion by the Rite of Circumcision.*—Dr. R. W. Taylor, in the early part of last year, was requested by the Board of Health of New York to investigate four cases in which it was suspected that syphilis had been communicated through circumcision. Four Jewish children who had been circumcised by a Hebrew named H— were attacked with phagedænic ulceration of the penis, and with lesions of the skin and lymphatic ganglia, accompanied, in three of the cases, with exhaustion, which resulted in death. The questions which arose, and which were of great importance in their social, sanitary, and medico-legal bearings, were—What was the nature of the disease with which these children were affected? In what manner was the disease communicated, or how did it originate? What means can be taken to prevent similar cases of disease in future?

The results of the inquiry were that, in one child, named Gutmann, there were the typical lesions of syphilis, and that the infection was possibly communicated in the performance of the rite, but that in the other cases it was doubtful if the symptoms were those of syphilis.

The theory suggested was that these Jewish children became syphilitic in consequence of the wound in circumcision having been sucked by the operator, who had syphilitic lesions of the mouth. An examination of the operator, H—, failed, however, to show that he was suffering from syphilis; and in the end Dr. Taylor reports that he must leave the origin of all the cases in doubt. He considers, at the same time, that the inquiry has been of service, and he offers the following conclusions:

1. That in the Jewish rite of circumcision there is a possibility of the occurrence of syphilis.
2. That the contagion is most likely to be communicated in the act of sucking the wound, the mouth containing a styptic liquid, and that perhaps it may occur by means of instruments soiled by syphilitic blood.
3. That the chances of such contagion are rendered greater by the performance of the operation by irresponsible, non-professional persons.

4. That the operation of sucking should be wholly abolished, and that, if a styptic solution of any kind is used, it should be poured from a vessel on the wound rather than squirted upon it from the mouth of the operator.

5. That in no instance should two or more children be thus operated on consecutively without a thorough cleansing of the instruments and utensils used after each operation, and that in every instance the greatest care should be taken in cleansing the instruments.

6. That the performance of the rite should be absolutely confined to responsible and educated persons, either a physician alone being selected, or a physician assisting an officiating rabbi, or a circumciser of recognised merit.

7. That under these circumstances accidents of any kind are reduced to a minimum.

Attention to these points will, under any circumstances, be of great benefit, and will render a rite which has useful sanitary bearings less liable to fall into disrepute among those upon whom it is obligatory.—*Reprint from the New York Medical Journal*, December, 1873.

#### IV.—SUMMARY:

*Report of the Metropolitan Board of Works for 1873; separate Report, published by the Metropolitan Board.*—The report for the year 1873 is more than usually valuable. The Board give sound reasons why the A B C utilization of sewage (alumina, blood, and carbon system) could not be adopted with any hope of profit to the ratepayers. There was not sufficient sale of the product to warrant the continuance of the process. In justice, however, to the A B C or Native Guano Company, it is stated that the condition of the effluent water was found, on the whole, to be very good, and no offensive effluvia or nuisance resulted from the preparation of the manure. The Board has prepared and approved a series of stringent sanitary rules for the management of private slaughter-houses.

*The Right Use of Disinfectants.* By H. LETHEBY, M.B., M.A. (A Reprint from 'Public Health,' 1873).—Dr. Letheby, in this paper, discusses the disinfecting properties of—1. The normal acids—sulphurous, nitrous, hydrochloric, sulphuric, and chromic. 2. Of the organic acids—carbolic, cresylic, acetic, picric, and benzoic. 3. Of the alkalies—lime, potash, soda, and ammonia. 4. Of the haloids—iodine, chlorine, chloride of lime, chloride of zinc, chloride of ammonium, chlor-alum, and common salt. 5. Of mineral sulphates—sulphate of zinc, sulphate of iron, common alum, and sulphate of copper. 6. Of permanganate of potash and chlor-ozone. 7. Of the volatile oils—camphor and turpentine. 8. Of charcoal and other porous substances. 9. Of air and water. 10. Of heat.

The application of these agents is next considered. For the disinfection of matters discharged from the sick body the author suggests that they should be received into a solution of two pounds of sulphate of iron (green copperas) dissolved in a gallon of water.



*Report of the Royal Edinburgh Asylum for the Insane for the year 1873.* By T. S. CLOUSTON, M.D., Physician-Superintendent. (Special Report from the 'Royal Asylum Press.')

—Dr. Clouston calls attention in this paper to one very interesting and important topic, viz. the cause of death in a lunatic asylum. From such facts as may be learned on this subject the physician may derive much light as to the nature of the diseases his patients have laboured under, the hygienic conditions of the institution, and the sufficiency of the diet. He shows that two causes of death call for particular attention; those are—exhaustion from acute insanity and pulmonary consumption. Excess of deaths from the first of these causes suggests a defect in the mode of treating the recent cases; excess from the latter cause usually indicates deficient food, too little fresh air, too great monotony of life, or all these combined.

*Composition of Extract of Meat.* By E. REICHARDT. ('Arch. Pharm.,' iii, 399—402.)—In this analysis M. Reichardt determined in the years 1870 and 1873 the exact composition of the extract of meat of commerce prepared at Monte Video by Messrs. Buschental. The author endeavoured by repeating his analyses to determine the varieties, in quality of different specimens.

The following is a summary of his researches.

|   | 1870.   | 1873.           |
|---|---------|-----------------|
| Substance soluble in alcohol (80 per cent.) | 81.00   | 80.15 per cent. |
| Water                                       | 16.50   | 15.92 „         |
| Fat and albumen                             | not any | not any         |
| Nitrogen                                    | 9.78    | 9.47 „          |
| Ash   | 21.36   | 21.30 „         |
| Phosphoric acid                             | 6.10    | 6.00 „          |
| Potash                                      | 8.87    | 9.00 „          |
| Soda  | 2.30    | 2.46 „          |

*Use of Aniline for Colouring Sausages.* By E. REICHARDT. (Ibid., ii, 574.)—Reichardt points out in this paper that the manufacturer, in order to give colour to inferior soluble matters used for sausages, sometimes adds aniline to represent the colour of the blood. Sausage meat thus tinted is charged with a substance which, habitually consumed, would prove very injurious to the health of the consumer, and in addition it may admit arsenic into the system as an accidental impurity of the aniline. To detect the fraud the meat should be finely minced and then digested with a mixture of alcohol and ether. In this solution the aniline, if it be present, will be dissolved and will give a tinted solution. Blood-colouring matter, on the other hand, if it alone be present, will not yield any soluble colour stuff to the solution.

*The Caisson Disease.* By ANDREW H. SMITH, M.D. (Prize essay of the Alumni Association of New York; Brooklyn, 1873.) In this able essay the author describes a disease depending upon increased atmospheric pressure, but always developed after the pressure is removed. The symptoms—those of neuralgia, of paralysis, or of cerebral character—are most carefully narrated. The duration of the caisson disease varies from three or four hours to six or eight days.

## REPORT ON PHYSIOLOGY AND HISTOLOGY.

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## LYMPHATIC SYSTEM.

1. J. ARNOLD, of Heidelberg. *The Lymph Canalicular System and its Relation to the Blood-vessels and proper Lymphatic System*, in 'Centralblatt für die Med. Wissenschaften,' No. 1, 1874.
2. SCHWALBE. *The Lymphatics of the Retina and of the Vitreous*, in the 'Leipziger Physiol. Arbeiten,' Band vii, 1872.
3. Dr. FRANZ MORANO, of Naples. *The Lymphatic Sheaths of the Vessels of the Choroid*, in 'Centralblatt f. d. Med. Wiss.,' No. 1, 1874.
4. ED. ALBERT and S. STRICKER. *Researches on the Temperature of the Heart and Lungs*, in the 'Wiener Med. Jahrbucher,' 1873.

1. Arnold remarks that recent researches on the phenomena of diapedesis have shown that the red corpuscles emerge through the vascular walls at the points where stomata exist, and that it is through these that vermilion suspended in solution of gum escapes into the tissues. Vessels which have been injected show slight projections at their points, and from these projections Arnold thought he was able to observe lines proceed, which in some instances had a ramified type. To examine the structures implicated more closely he tied the veins of various parts, as of the tongue and of the web of the feet in frogs, in order to produce venous hyperæmia and œdema, and then injected the parts so affected both by the blood-vessels and the lymphatics. A fine network then made its appearance, connected on the one hand with the blood-vessels, and on the other with the true lymphatics, which presented dilatations at various points. This system presented a very regular arrangement around and in glandular organs, and it is this system which, greatly distended, causes the swelling in venous states and œdema. (See note on Schiff in present number of this Journal, (p. 264), in regard to cause of œdema).

2. Schwalbe puts a ligature round the optic nerve, and enters the point of an injection syringe through the inner sheath of the nerve, between the place of application of the ligature and the bulb of the eye. The injection thrown in is turpentine coloured with alkanet or Prussian blue held in suspension in water. By this means the perivascular spaces of the retina are filled, both the veins and capillaries being thus surrounded by a sheath, but the arteries never exhibiting any. The injection occupying the perivascular spaces is only separated from the lumen of the vessel by a delicate tube of endothelial cells. The injected fluid extends from the papilla of the optic nerve to about the distance of four millimètres in a radiating direction in the optic fibre layer, and the spaces thus filled present



numerous smooth cells like an endothelium. The injection further penetrates between the vitreous and the limitans retinæ, and finally the central canal of the vitreous is in some instances filled. Since in these instances the injected fluid must exude through numerous fine pores of the inner optic nerve-sheath, and thus gains the intervaginal space, which is a well-recognised lymph space, the connection of the vessels injected in the vitreous and retina with the lymphatic system is demonstrated.

3. Dr. Morano examined the choroids of sheep, rabbits, cats, dogs, and of man, which had been emaciated in Beale's fluid, and found that capillary vessels, the walls of which were well defined in consequence of being filled with injection of Berlin blue, were distinctly united by lymphatic sheaths. The proper vascular wall presented oval or roundish nuclei, whilst the lymphatic sheaths were composed of fusiform or elongated corpuscles containing much protoplasm, and connected with each other by processes. The connective-tissue-corpuscles of the choroid are directly connected with the just described corpuscles of the lymph sheath, but are fusiform and very large, with islands of protoplasm rich in pigment in their interior, and obviously containing tubular canals. These constitute transitional structures between lymphatics and blood-vessels.

4. Albert and Stricker observe that little reliance can be placed upon the ordinary measurements of the temperature within the heart, because the measuring instrument must necessarily touch both the tissue of the heart and the blood, the temperature of which might differ considerably. They therefore embedded one in the substance of the left ventricle, and found that the temperature was from 0·5 to 0·7 C., higher than when it was only in contact with the inner wall of the ventricle. The same experiment made on the right ventricle indicated much less difference. A thermometer introduced through the jugular vein into the right ventricle showed that the blood in the right ventricle was warmest near the apex, and warmer everywhere than that of the right auricle, whilst this, again, was warmer than the blood of superior vena cava. They attribute the increased temperature of the blood in the ventricle to the admixture of the blood of the coronary vein which has circulated through the tissue of the heart. They find further, as many experimenters have done before them, that the blood is somewhat cooled in passing through the lungs, so that the blood of the left side of the heart is cooler than that of the right.

#### GLANDS.

1. LATSCHENBERGER. *On the Structure of the Pancreas*, in the 'Wiener Akad. Sitzungsberichte,' 1872, B. 65, p. 195.
2. Dr. GEORG. SALOMON. *Ueber die Bildung des Glycogens in der Leber. Vorläufige Mittheilung*, in 'Centralblatt f. d. Med. Wiss.,' 1874, p. 179.
3. B. LUCHSINGER. *Zur Glycogenbildung in der Leber*, in 'Pflüger's Archiv,' Band vii, p. 289.

4. P. A. BOÉCHAT. *On the Structure of the Thyroid Gland.* Pamphlet, 1873, pp. 44.
5. K. MÜLLER. *On the Influence of the Skin on the Secretion of Urine*, in 'Archiv f. Experim. Pathol.,' 1873, p. 429.
6. A. MORRIGGIA. *On the Urine and Sweat*, in 'Moleschott's Untersuchungen,' 1873, No. 9.
7. H. BRAUN. *Ueber den Modus der Magensaft-Secretion*, in 'Eckhard's Beiträge zur Anatomie und Physiologie,' Band vii, p. 72.
8. J. C. DALTON. *On the Spectrum of Bile*, 'in New York Med. Journal,' vol. xix, No. 6, p. 579, 1874.

1. The fine plexuses and radiating canals embracing the alveoli of the pancreas, injected and described by Giannuzzi, Saviotti, and others are, according to Latschenberger, only portions of injection which have penetrated between the epithelial cells. Quite similar plexuses and canals may be demonstrated by injection between the epithelial cells of the excretory ducts. The pancreas is not, he thinks, an acinous gland, but is of the type of a branched tubular gland, the elements of which curl round and compress one another, the intermediate spaces being filled with connective tissue and blood-vessels. He has never been able to see the pyriform anastomoses of the excretory ducts described by Giannuzzi. He regards the gland-cells as destitute of a membrane. The intra-acinal cells described by Langerhaus he holds with Sertoli to be the ultimate cells of the excretory ducts.

2. Dr. George Salomon states he has been engaged in experimenting on the formation of glycogen in the liver for several months past, and he now proposes to give a short account of the results he has obtained. His researches were conducted on rabbits, and consisted in the administration of certain articles of diet and the determination by analysis of the amount of glycogen contained in the liver. The method adopted for the latter purpose was in accordance with a plan suggested by Brücke, except that the liver was not boiled with potash. Dr. George Salomon is in accord with Luchsinger in regard to the great difficulty of completely extracting both the glycogen and sugar from the liver, and he further remarks that he constantly found small quantities of sugar present in the alcohol used for the precipitation of the glycogen after removal of the mercury by sulphuretted hydrogen. Before each experiment the animals were made to fast from forty-eight to eighty-four hours, and the following substances were then given:—(1) gelatine, (2) olive oil, glycerin, and a salt of one of the fatty acids, and (3) carbohydrates (cane, milk, fruit sugar, and mannite).

In the first set of experiments animals fed on gelatine furnished the following quantities of glycogen: 0·588—0·707—1·150—0·520—0·802—0·366—0·5 results, which showed the correctness of Hoppe Seyler's statement, that with gelatine diet glycogen is found in the liver. Gelatine-glycogen presented the same chemical reactions as ordinary glycogen; its strongly opalescent solution when treated



with dilute acids easily became converted into sugar, polarising to the right. In the experiments with oleic oil, glycerine, and salts of the fatty acids, six animals were fed on pure olive oil. Their livers furnished 0.216—0.430—0.698—0.225—0.088—0.365. Neutral fats therefore form glycogen, if in small quantities. Four animals were fed on diluted glycerine, and the abundant glycogen found in the liver substantiated the observations of Weiss and Luchsinger. Two animals were supplied, the one with 108 grains, the other 200 grains of soap. In the liver of the former was 0.25 grammes of glycogen, in the latter only traces. No certain conclusion can be drawn from this experiment, though it is probable that only the glycerine in its neutral fat acted as the glycogen-forming part.

In the experiments with the carbohydrates Salomon was able to corroborate Doelis's statement in regard to the formation of glycogen after cane sugar diet. In animals to which milk sugar was given the liver furnished in glycogen 0.067—0.095—0.43—0.258—0.533—0.952—2.03—0.247—0.873—0.032. Glycogen is therefore formed on milk sugar diet, as Luchsinger had previously rendered probable. Two animals fed on glucose obtained by warming inulin with diluted sulphuric acid gave 1.647 and 1.665, the results again agreeing with those obtained by Luchsinger. Eleven animals were fed on mannite. Their livers gave of glycogen 0.245—0.037—0—0.012—0.059—traces—0.076—0.078—0.129—0.143. It can then be stated with certainty that little or no glycogen is formed on a diet of mannite. In a final series of experiments Salomon tried the effects of the administration of monacetyl saccharose with the view of obtaining an acetyled glycogen, and a proof of the direct formation of glycogen. It appears, however, that the substance is broken up in the animal economy, and only normal glycogen is found in the liver.

3. Luchsinger has repeated the experiments of Weiss on the effects of adding glycerine to the diet, and tried various other easily oxidizable materials. He sometimes employed fowls and sometimes rabbits. After injecting on one occasion 42 grammes and on another 60 grammes of glycerine into the stomach, Luchsinger found 0.55 and 0.71 grammes of glycogen in the liver, whilst after injection of 50 grammes of grape sugar there were 1.678 grammes, and therefore a considerably larger proportion, notwithstanding that sugar, according to the researches of Scheremetjewski, was oxidizable, with great difficulty, or in other words was less capable of screening the glycogen of the liver from oxidation. By testing the power of circular-polarization of the glycogen stored up in the liver after addition of glycerine to the food it was ascertained to be identical with ordinary glycogen. In the experiments on rabbits the same results were arrived at, the injection of glycerine causing abundant accumulation of glycogen in the liver and in the muscles also. The injection of glycogen subcutaneously (50 cc.m. being injected) caused only traces to be recognised in the liver, whilst none was present in the blood-vessels. An experiment with fat diet had a negative result, the liver containing a large proportion of fat, but no glycogen. It would appear from this that notwith-

standing the abundance of easily oxidizable material in the body, the glycogen was not preserved from oxidation. Similarly, researches with tartrate and lactate of soda showed either no glycogen in the liver or only traces. Luchsinger now set himself to determine whether the use of other kinds of sugar would cause the accumulation of special kinds of glycogen. Experiments made with milk sugar and inulin caused the storing up of glycogen in the liver, and careful testing showed that the glycogen so accumulated was identical with ordinary glycogen. Upon the whole Luchsinger is disposed to believe that the glycogen of the liver proceeds from the carbohydrates.

4. M. Boéchat recommends, as the best mode of preparing the thyroid for examination, that it should be hardened in solution of picric acid, then left for a few days in solution of gum, and finally immersed in alcohol until fit for section. The alveoli of the gland, he maintains, are not closed roundish vesicles, but irregularly shaped cavities which freely intercommunicate. The walls are formed of a single layer of epithelial cells, by which in many places they are alone separated from the lymphatic spaces. The individual cells are broader than they are high. In adult life the cavities of the gland are filled with colloid substance, which stains with anilin and with carmine. The lymphatic sinuses of the thyroid form a wide cavernous plexus contained in the interior of the connective tissue of the stroma. In the interstices of the lymphatic lie the alveoli and the blood-vessels of the gland.

5. Müller investigated the effect of excitation of the skin on the secretion of urine in Bernard's laboratory, using large dogs. The hair was shaved off, and the urine collected from both ureters and registered by a special instrument. Six experiments were made with ice-cold cloths placed round the animal. In the course of from ten to twenty minutes an increase of from 23-50 per cent. occurred. Three experiments were made with warm cloths, and a diminution of from 50-73 per cent. noted. In three other cases water at a temperature of 104° Fahr. was poured over the animal, and a diminution of 83 per cent. was observed. Friction applied to the skin gave varying results; mustard poultices and blistering paper caused no noticeable change in thirty minutes. Varnishing of the skin caused slight diminution of the quantity of urine secreted in 5 out of 7 cases.

6. The difference in the reaction of the urine in carnivora and herbivora suggested to Morriggia to undertake some investigations to ascertain whether a corresponding difference existed in regard to the sweat or excretion that offers so many points of analogy to the urine. Experiments on himself and on animals satisfied him that the sweat of herbivora is usually alkaline, that of carnivora usually acid. In both man and animals the urine becomes highly acid during fasting or when confined to flesh diet, while it is alkaline on vegetable diet. The sweat, however, retains the reaction peculiar to the individual whatever may be the nature of the food. Morriggia finds that it takes a much longer time to change the reaction of the



urine from acid to alkaline, or *vice versâ*, by alteration of the diet than is usually supposed.

7. The view generally accepted of the mode in which the gastric secretion takes place is that it is excited by a stimulus either of a mechanical or of a chemical nature. In opposition to this Braun states that his experiments lead him to the belief that the gastric juice, like the urine, is secreted continuously. He experimented on dogs in which he had made a gastric fistula. Through these various irritating substances, as sponges, the vanes of feathers, stones, alkalies, and pieces of meat were introduced; but he found that no variations occurred in the amount poured forth. The alkaline saliva of dogs and men had no greater influence, nor did there appear to be any relation between the amount of alkali excreted in the saliva and of acid in the gastric juice, for neither did the irritation of the salivary gland have any influence on the secretion of gastric juice, nor when this last was excited to flow was there any change in the amount or characters of the saliva. Braun opposes the hypothesis of Schiff, that pepsine is produced from peculiar, so-called peptogenous bodies in the stomach, since even in fasting animals whose salivary ducts are divided a not inconsiderable quantity of gastric juice can be obtained. He is much rather disposed to agree with Spallanzani, that the pepsin and acid are continuously excreted. The mucous membrane of the empty stomach is only rarely covered with a tenacious mucus, but much more frequently with a fluid that usually has an acid reaction.

The gastric juice, like the urine, is secreted in large quantities when water is injected into the femoral vein, and this is not a simple case of transudation, but the fluid exhibits an acid reaction, and digests albuminous substances; at the same time it was often requisite, in order to give the fluid a digestive power, to add some hydrochloric acid to it, while M. Braun associates with the observation of Manassein, that the acid of the gastric juice is deficient in acutely anæmic animals.

8. Dr. Dalton believes that the bile presents a very distinct and characteristic spectrum. He has examined the fresh bile of the ox, sheep, pig, dog, and man. In indicating the measurements of the spectrum he employs Vierordt's method. This consists in taking as fixed points the eight principal lines of the solar spectrum from A to H. The spaces between these lines are then considered as divided each into 100 equal parts, and the situation of an absorption band is expressed by proportional numbers, counting in the direction from the red to the violet end of the spectrum. The first distinguishing character of the spectrum of bile is that it is *very short*—the light being totally absorbed at a considerable distance from the refrangible end. In specimens of ox bile, viewed in a thickness of one centimetre, in no case did the spectrum extend beyond the line F in the first quarter of the blue, and in most instances fell considerably short of it. In the human subject it was shorter still. As a general rule the spectrum was characterised by an absorption band at c. The existence and intensity of this band are proportional to the

predominance of green in the colour of the bile. The spectrum of the bile is also distinguished by a diminution or absence of the orange and yellow, and a corresponding extension of the red and green. There are sometimes also two other absorption bands, comparatively uncertain and ill-defined, at D and at  $D_{30}E$ . The pure biliary salts in alcoholic solution treated by Pettenkofer's test give a spectrum with absorption bands at E and F. In a watery solution treated by the same test they give a spectrum with but one absorption band, namely, at E.

#### NERVOUS SYSTEM.

1. KEY and RETZIUS. *On the Structure of the Nervous System*, in Max Schultze's 'Archiv f. Microscop. Anatomie,' 1873, B. ix, p. 308; and 'Cbl.,' No. 3, 1874.
2. THOMAS M. RUMBOLD. *The Function of the Eustachian Tube*. 1873. Pamphlet.
3. PH. SCHECH. *Experimentelle Untersuchungen ueber die Functionen der Nerven und Muskeln des Kehlkopfes*, in 'Zeitschrift für Biologie,' Band ix, p. 258, 1873.
4. E. MANDELSTAMM. *Ueber Schnerven Kreuzung und Hemiope*, in 'Archiv f. Ophthalmologie,' Band xix, p. 39.
5. MICHEL. *Ueber den Bau des Chiasma nervorum opticorum*, in idem, p. 59.
6. BROWN-SÉQUARD. *Recherches sur les communications de la rétine avec l'encephale*, in 'Archives de Physiologie,' 1872, p. 261.
7. Prof. M. SCHIFF. *Des Nerfs Cardiaques*, in pamphlet 'De l'Inflammation et de la Circulation,' 1873, p. 96.
8. ALBERT H. BUCK. *On the Mechanism of Hearing*, a very interesting paper in the 'New York Med. Journal.' No. for June, 1874. See also—
9. E. CYON. *Ueber der Function der halbzirkelförmigen Canälen*, in 'Pfluger's Archiv,' Band viii, p. 306. And—
10. J. BREUER. *Ueber der Function der Bogengänge des Ohrlabyrinthes*, in 'Wiener Med. Jahrb.,' 1874, 1. p. 72.

1. MM. Key and Retzius begin their account with a careful description of the microscopical characters of the spinal cord, dura mater, subdural space, arachnoid, subarachnoid space, ligamentum denticulatum, and septum posticum. Minute details are, in particular, given of the subarachnoid trabeculæ, which have an important bearing on the connective-tissue question. These trabeculæ, which were examined after maceration in osmic acid and staining with anilin, consist of fine fibrils, and are all, without exception, more or less closely surrounded by a thin uniting membrane. This sheath is homogeneous, except that nuclei are distributed through it at certain points, causing both of its surfaces to project at these places. Around the nuclei the membrane is granular. It is clearly composed of a layer of extremely delicate cells (endothelial cells), the outlines of which are well brought into view by nitrate of silver. In newborn animals the sheaths are more protoplasmatic than in younger



ones; when treated with acetic acid the more delicate trabeculæ spring back, and their annular remains produce the appearance known as "spiral fibres."

The trabeculæ are disposed in a plexiform manner at certain points, in consequence of the interchange of fibrils, and the cell investment expands at the angles of this plexus and over the spaces between the trabeculæ in the form of delicate membranes.

It can easily be understood how a double layer of cells can thus be formed, and a key is thus given for the solution of one of the most contested points in the connective-tissue theory, namely, the explanation of a double layer of flattened cells (the contours of which can be rendered very distinct with nitrate of silver), in the middle of or between which are fasciculi of fibrils arranged in a plexiform manner. The authors distinguish two layers in the pia mater, of which the internal is nearly the same in all animals, whilst the external presents great variations. The external is composed of longitudinal fibres, running parallel to one another, and their fibrillated membranes. The ligamentum denticulatum is intimately connected with this outer layer. The internal layer is divisible into three laminæ, an external limiting cell-membrane with a more elastic longitudinal disposed plexus, a middle layer with stiff circular connective-tissue fibres, and an internal cell-membrane separating it from the spinal cord, also possessing an elastic circularly arranged plexus.

This internal layer is in direct contact with and slightly adherent to the neuroglia of the spinal cord, so that no space exists between them; it is continued on the outside of and around the vessels entering the cord forming their sheaths. The conclusion in regard to the minute structure of the nerves arrived at by Key and Retzius agree, upon the whole, with those of Ranvier.

Each of the nerve-fasciculi composing a great trunk like the sciatic is seen to be surrounded by a broader or narrower well-defined ring, which is equivalent to Robin's perineurium. The connective tissue in the interior of this is named by these observers endoneurium, and that external to it epineurium. In injected nerves the injections take the place of the perineurium, and close investigation shows that it has run between fine membranes of which the perineurium is composed. These last closely resemble arachnoidal membranes, and are, in fact, a more or less direct continuation of the arachnoid. The endoneurium is in very intimate connection with the perineurium, whilst several perineural membranes separate off from the perineurium and penetrate into the interior of the nerve-fasciculus. It thus becomes intelligible how with strong pressure the injection easily penetrates from the perineurium into the endoneurium, and ultimately causes the several primitive nerve-fibres to be separated by the injection.

In regard to the ultimate nerve-fibres, the authors describe them as being invested, besides the sheath of Schwann, with a second comparatively wide tube, composed of delicate endothelial cells and connective-tissue fibrils. Injection fluid forced in from the perineurium into the endoneurium spreads itself, not only between the fibrillated

sheaths of the several nerve-fibres, but penetrates also into the interior of these fibrillated sheaths, and bathes the sheath of Schwann of the primitive nerve-fibres. It never, however, runs internal to the sheath of Schwann. It is highly probable that the spaces above described are the lymph-paths of the nerves.

The nerve-fibres, therefore, each surrounded by the sheath of Schwann, lie within a fibrillated sheath, between which and the sheath of Schwann is a layer of lymph. This communicates by opening with the spaces of the perineural sheaths, and by means of these with the lymph spaces of the central venous system. This lymphatic system is throughout the whole peripheric nerve system entirely shut off from the ordinary lymphatic system of the body at large, although it assumes the plexiform structure of the same in the spinal ganglia.

2. Dr. Rumbold endeavours to prove the correctness of the following proposition:

a. That during the act of deglutition the Eustachian tube is not an open passage into the tympanum.

b. That the walls of the Eustachian tube are constantly in slight contact.

c. That the air continually permeates the Eustachian tube into the tympanum, thus maintaining the normal air density in this cavity.

d. That the air in the normal tympanic cavity is not of equal density with that of the surrounding atmosphere, the air in the tympanum being rarefied.

e. That one of the functions of the Eustachian tube is the maintenance of this normal air density.

f. That the rarefied condition of the air in the tympanum is the cause of the uniform concavity of the membrana tympani, especially that portion of it from which the light spot is reflected.

The evidence adduced in favour of these several propositions is chiefly derived from clinical observation.

3. P. Schech conducted his experiments on young and middle-sized dogs, which were fastened down, narcotised by means of morphia, and their mouths widely opened by means of a dilator. The tongue was then drawn forward, and the movements of the larynx examined with a laryngoscope. The motor nerves of the larynx have been by some believed to be derived from the vagus, by others, and, indeed, by the majority of experimenters, from the spinal accessory. Schech gives an excellent historical account of the various opinions that have been held on this point, and on the evidence obtained from experiment and from pathology. His own experiments lead him to the conclusion arrived at by Bischoff that it is the spinal accessory nerve which supplies the motor fibres to the larynx. Extirpation or eradication of the accessory nerve of one side causes immobility of the vocal cords on that side, whilst evulsion of both accessories causes immobility of both vocal cords in cadaveric position and complete aphonia.

Experiments on the *Superior Laryngeal Nerve*.—This branch



divides near the great cornu of the hyoid bone into a large internal branch and a smaller external branch. The former is sensory, the latter motor. Supplying the crico-thyroideus, section of the superior laryngeal nerve prior to its division or even of the external branch alone, prevents the longitudinal tension of the vocal cords, causes the production of a hoarse and deep voice, and abolishes the power of making high notes.

*Action of the Crico-thyroid Muscle.*—The generally accepted view is that the crico-thyroid draws the thyroid cartilage forwards and downwards, and therefore elongates and tightens the vocal cords. But in opposition to this is the fact that in the production of high notes the thyroid rises towards the hyoid. The elevation and fixation of the thyroid is effected by the contraction of the thyrohyoids; the crico-thyroid, therefore, in order to effect the intended tension of the vocal cords, nothing remains but to oppose the thyrohyoid with all its might, to which, however, its power is very inadequate. He thinks the action of the crico-thyroid is best explained on the following view:—When the thyroid cartilage is fixed by the thyrohyoids the crico-thyroids draw the anterior part of the cricoid upwards towards the thyroid, and then effects a rotation of the plate of the cricoid backwards and downwards. Now, since the thyroid cartilage is fixed anteriorly by the thyrohyoids, whilst the arytenoid cartilages are fixed posteriorly by the arytenoidei transversi and crico-thyroidei laterales to the cricoid cartilage, the backward inclination of the body of the cricoid caused by the contraction of the crico-thyroid affects also the arytenoid cartilages resting on it; the distance between the anterior and posterior attachments of the vocal cords is increased, and the longitudinal tension of these cords is brought about.

*Function of the Nervous Laryngeus Inferior.*—This is the principal motor nerve of the larynx, supplying all its muscles with the exception of the crico-thyroid. The constant results of section of the recurrent nerves on both sides are complete aphonia, immobility of the vocal cords, constriction of the glottis, and absence of dyspnoea in the state of rest. The vocal cords take up the position they assume in death, since all the muscles are rendered inactive, and none, therefore, preponderate in their action over the others.

*Function of the Crico-arytenoideus Posticus Muscle.*—This is the most important of all the muscles of the larynx. It is the dilatation of the glottis, and its venous supply, though this has not been demonstrated, is probably partly derived from the vago-accessory centre and partly from the respiratory centre. When divided on both sides the vocal cords approximate one another, and are no longer capable of moving outwards or separating from each other during efforts at respiration. Complete approximation and persistent dyspnoea do not occur immediately. The contraction of the glottis and the vibration of the vocal cords occur normally and the voice remains wholly unaltered.

4. Mandelstamm and Michel have independently of each other arrived at the conclusion that the decussation of the optic nerves in

the chiasma is complete, which is in accordance with a previous observation by Brown-Séquard. Michel's investigations were chiefly made on horizontal sections of chiasmata hardened in chromic acid, and examined under the microscope. The mode of decussation varies in different animals. In fishes the opposite nerves are simply superimposed on one another; in amphibia and birds the decussation is lamelliform. In frogs the bundles of fibres interweave like the fingers of the two hands when they are locked. In birds the decussation is similar. In the mammals the fibres form a delicate web of interweaving fibres, like basketwork or straw-plaiting. The prolongation of the pia mater on the fasciculi ceases where the decussation commences, and the fasciculi then becomes much finer. Both Michel and Gudden have observed cases of congenital atrophy or non-development of one eye in which the optic nerve of the same side and the optic tract of the opposite side were alone atrophied. In man there is a thin posterior commissure which is separated from the chiasma by grey matter, and extends for only a short distance along the tracts.

Mandelstamm chiefly adopted the method of teasing out the fibres after treatment with chromic acid, alcohol, and strong solution of potash. His description corresponds fairly with that given by Michel. He found in addition that section of the "anterior corpus quadrigeminum" was followed after some weeks by marked atrophy of the opposite retina, whilst the other eye remained unaltered.

Brown-Séquard's results were identical with those of Mandelstamm. Section of one optic tract caused amaurosis of the opposite side, sagittal section of the chiasma complete blindness. In rabbits and guinea-pigs injury of many parts of the brain causes amaurosis of the opposite eye; whilst on the contrary injury of the medulla oblongata and spinal cord causes amaurosis on the same side.

John Reid first observed that, after section of the two vagi in the neck, the pulse does not remain uniform, but that on the contrary the movements of the heart are still susceptible of modification by physical and psychological excitation.

Schiff repeated Reid's experiments in 1848-49, and then maintained that the vagus is not the only, though it is the chief excitator nerve of the heart—the accelerator fibres alone being at that time known. He attributed the increased frequency of pulsation observed in animals deprived of their vagi during any excitation applied to the nervous system generally, to a nervous influence acting directly upon the heart, and applied himself to ascertain the existence and course of the fibres implicated. At a subsequent period, when Ludwig and Thiry had shown that an augmentation of the arterial pressure usually increased the frequency of the pulse, it seemed to him probable that the elevation caused by general irritation of the nervous system after section of the vagi was due to such augmentation of the arterial pressure. But as all experiments undertaken with a view of establishing the presence of proper accelerator nerves of the heart proved futile, and it was demonstrated in particular that the sympathetic nerve had no excitator action, it became more and more probable that



the vagus was the only motor cardiac nerve. Physiology possessed no means of rendering the frequency of the pulse independent of the pressure. Schiff, however, in the course of recent investigations, has discovered the interesting fact that atropine given in small doses destroys all relation between the pressure of the blood and the rhythm of the cardiac beats. It had long been known that atropine paralyses the inhibitory nerves of the heart contained in the vagus, but it now appears that by means of a few drops of a concentrated solution of this drug the heart can be rendered so insensible to variations of pressure that this can augment to double or triple its ordinary amount without the occurrence of any alteration in the frequency of the beats. Hence, admitting that the vagus is the sole motor nerve of the heart, and that the increased rapidity of the pulse from general excitation of the nervous system after section of the vagi depends on augmentation of the blood pressure, *atropinisation* in addition to section of the vagi ought to render the pulse regular and invariable. Experiments conducted in the laboratory at Florence did not, however, confirm this conclusion, and after a long series of researches bearing upon the point, Schiff has not only been able to demonstrate the existence of excitor nerves of the heart, but also to follow their devious tract. These nerves belong to the system of the vagus and have their roots in the accessory nerve. The excitor fibres enter the vagus at the base of the cranium, and in the dog separate from the vagus again before the issue of this nerve from the second ganglion or ganglion of the trunk, which corresponds to the gangliiform plexus in man, in order to unite with the cardiac plexus. Audersch long ago was aware that the superior laryngeal nerve gave off a small anastomosing branch to join a ramification of the inferior laryngeal nerve. It is probable that the cardiac nerves follow the course of this anastomosis to run in the recurrent as far as the thoracic cavity, and there pass off in the delicate branches which this nerve gives off to the cardiac plexus known to Audersch and Neubauer, and which re-enter the trunk of the vagus to emerge again almost immediately with the last cardiac branches.

#### DEVELOPMENT.

1. W. H. DALLINGER and J. DRYSDALE, M.D. *Researches into the Life-History of the Monads*, in the 'Monthly Microscopical Journal,' January 1, No. lxi, 1874.
2. FR. MERKEL, of Rostock. *The first stages of development of the Spermatozooids*, in the 'Centralblatt für die. Med. Wiss.,' No. 5, 1874.

M. Dallinger and Dr. Drysdale state that in pursuing their researches they now become more and more convinced of the importance of prolonged and patient examination of the same forms. Two observers examining the same monad independently of each other, if these inquiries were not sufficiently prolonged, might with the utmost truthfulness of interpretation assert opposite modes of

development. They give an account of the metamorphosis undergone by various forms of monads, and then describe some experiments bearing on the question of Abiogenesis. Their researches, they hold, show conclusively that the assumption the germs of putrefactive organisms must perish in the same conditions in regard to temperature that destroy the parents is erroneous.

2. In 1871 Merkel and Von Ebner simultaneously published essays upon the development of the spermatozoa. Von Ebner was of opinion that the spermatozoa were parts of the protoplasm of the so-called spermatoblasts, whilst Merkel held that they proceeded from the round cells of the testes, and regarded the spermatoblasts of Von Ebner as fission cells (*stückzellen*).

In consequence of this difference of opinion Merkel undertook further researches, and has satisfied himself that his own formerly expressed statements are the most correct. He is confident that the round clear cells of the testis before they lie in the pockets of the *stückzellen* have already passed through the first stages of development. The nucleus is the part which shows the first changes, one hemisphere of it becoming more fluid, and showing a great thickening of its membrane accompanied by the formation of a small capitate elevation, which, rising from the wall of the nucleus, projects into the protoplasm of the cell. As soon as the clear testicular cells are thus altered they form with the supporting cells the cell stocks (*zellenstöcke*), and then begins the conversion of the nucleus into the head of the spermatozoone. The changes can best be seen in preparations mounted in aqueous humour.

#### BOOKS, PAMPHLETS, &c., RECEIVED FOR REVIEW.

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The Hygiene of Schools, or Education Mentally and Physically considered. By J. B. Budgett, M.D. London, H. K. Lewis. 1874. pp. 88.

Rabies and Hydrophobia: their History, Nature, Causes, Symptoms, and Prevention. By George Fleming. With Illustrations. London, Chapman and Hall. 1872. pp. 405.

An Épitome of Therapeutics, being a comprehensive summary of the Treatment of Disease as recommended by the leading British, American, and Continental Physicians. By W. D. Stone, M.D. London, Smith, Elder and Co. 1874. pp. 359.

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Guy's Hospital Reports. Edited by H. G. Howse, M.S. Third Series. Vol. XIX. London, Churchills. 1874. pp. 555.

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The Hunterian Oration, Presidential Addresses, and Pathological and Surgical Writings. By Cæsar H. Hawkins, F.R.S. In Two Volumes. London, printed by Golbourn. 1874. pp. 448 and pp. 452.

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On Diseases of the Skin, including the Exanthemata. By F. Hebra, M.D., and M. Kaposi, M.D. Vol. III. Translated and edited by Waren Tay, F.R.C.S., for the New Sydenham Society. London. 1874. pp. 398.

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Lund, F.R.C.S. Manchester, Cornish. 1874. pp. 99.

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THE  
BRITISH AND FOREIGN  
MEDICO-CHIRURGICAL REVIEW.

OCTOBER, 1874.

Analytical and Critical Reviews.

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I.—Recent Psychological Doctrines.<sup>1</sup>

It will be seen by our list of recent works on mental science and philosophy that the great problems of life and thought have still, as ever, a deep interest for thinking men. The soldier and lawyer jostle philosophers (of both sexes, for we might have added Miss Cobbe's name to our list), while the physiologist, physician, and specialist labour to reveal the burden and the mystery of human life—with the greater fitness because it is their vocation. The books we note may be divided into two groups, viz. as they are scientific or positive, and philosophical or speculative; while the latter may again be divided, as they are logically and mystically philosophical, and the

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<sup>1</sup> 1. *Principles of Mental Physiology: with their Applications to the Training and Discipline of the Mind and the Study of its Morbid Conditions.* By WILLIAM B. CARPENTER, M.D., &c. &c. London, 1874.

2. *Body and Mind: an Inquiry into their Connexion and Mutual Influence; specially in reference to Mental Disorders, &c.* An enlarged and revised edition. By HENRY MAUDSLEY, M.D. &c. London, 1873.

3. *Responsibility in Mental Disease.* By HENRY MAUDSLEY, M.D. &c. 1874.

4. *On Certain Disorders and Defects of Organic Memory.* By THOMAS LAYCOCK, M.D. &c. 'Edinburgh Medical Journal,' April, 1874.

5. *The Physical Basis of Mental Life.* By R. R. NOEL. 8vo. London, 1873. Pp. 74.

6. *Mind and Body: the Theories and Principles of their Relation.* By ALEX. BAIN, LL.D. 8vo. London, 1874. Pp. 196.

7. *Problems of Life and Mind.* By GEORGE HENRY LEWES. First Series: *The Formation of a Creed.* 8vo. London, 1874. Pp. 472.

8. *A Treatise on the Nature of Man regarded as Triune: with an Outline of the Philosophy of Life.* By THOMAS BEST WOODWARD. 8vo. London, 1874. Pp. 277.

9. *What am I? a Popular Introduction to Mental Philosophy and Psychology.* By EDWARD W. COX, Serjeant-at-Law. 8vo. London, Vol. I, 1873; Vol. II, 1874.

former as they are more or less philosophical as well as positive and scientific.

Amongst the mystical works we place those of Mr. Cox and Mr. T. B. Woodward; amongst the philosophical and speculative, yet partly physiological, those of Dr. Bain and Mr. Lewes; amongst the positive and scientific, but yet partly philosophical, those of Dr. Carpenter, Dr. Maudsley, and Dr. Laycock, while Major Noel's modest, yet excellent and well-reasoned exposition of the principles of phrenology brings up the rear.

Since, broadly viewed, these works indicate the results of two distinct methods of inquiry, we shall examine them in their results, according as the one or other method is followed, and commence with the scientific and positive work of the day, viz. that by Dr. Carpenter, which contains, however, much that is speculative, but which will be very generally welcomed, if it were only as a mature exposition and expansion of the outlines of psychology contained in the fourth (1852) and fifth (1855) editions of his 'Principles of Human Physiology.' Besides these, it contains reviews, papers, and lectures (or *excerpta* from them), written subsequently to the latter date, so that the volume has swelled out to 737 pages. This size rendered it unfit to be associated with the rest of the "international series," as was intended. Like many works lately published as republications, it wants that unity of style, method, and doctrine which an entirely original work would probably have exhibited. The twenty chapters into which the book is divided are, however, full of interesting facts and suggestive discussions; more especially, as might be expected, the doctrine of the reflex function of the brain has numerous applications in explanation of the intellectual processes, whether of the reason or the imagination. In particular, that adaptation of it to "unconscious cerebration" which Dr. Carpenter favours has a full development.

From the subjoined extract we gather that the philosophical systems promulgated since 1855, and more especially in this country by Mr. Darwin, Mr. Herbert Spencer, and Dr. Laycock, not to mention continental systems of older date, like those of Comte, and of Hegel and other Germans, have not influenced the views he formed in 1852 and 1855.

"Not having seen reason to make any important change in my own Psychological views since I first put them forward, but, on the contrary, having found them confirmed and extended by the experience of twenty years, I set myself down to revise my former exposition of them, with the idea of simply introducing such illustrations as might lead to the more ready apprehension of the principles I aimed to enforce, and of filling up such deficiencies as



it might seem desirable to supply. \* \* \* I now send it forth as a contribution to that Science of Human Nature which has yet (as it seems to me) to be built up on a much broader basis than any Philosopher has hitherto taken as his foundation." (Preface, p. vii.)

Considering what has been lately done for psychology in biology and biological philosophy, Dr. Carpenter's judgment as to the narrow bases of all the hitherto current systems is, we venture to think, not well founded. As to his own method the criticism of Mr. Lewes seems to apply closely; it is not comprehensive enough. The philosophy of evolution has meagre uses. The physics of sensation, as elucidated by Helmholtz and others, might have been usefully considered in relation to reflex cerebral activity, and the true character of what is included under metaphysics more clearly set forth. In attempting to occupy the border-land between the latter and physiology without first discriminating, what is included under each, a fundamental error in method is made, the effects of which run through the whole treatise.

This error enters into the first chapter which treats of the "General Relations of Mind and Body"—the crucial question of any system of mental science based on physiology. Admitting the primary fact of experience that all mental states depend on correlative states of the brain, and consequently all those mental states known as will, Dr. Carpenter, instead of fixing his attention on the laws of life and organization, according to which the correlative vital states of brain are regulated and produced, adopts the method of speculative philosophy, places the will outside and above the brain deals with that wonderful mechanism as if it were "matter," in the speculative sense of the word, and more than intimates that the converse or biological method leads on to downright "materialism." We subjoin the terms in which Dr. Carpenter states this problem—

"Although few (if any) Philosophers would be disposed to question that the Brain is the instrument of our higher psychical powers, the ideas which are entertained of the nature of this instrumentality have been seldom clearly or consistently defined. Some, who have attended exclusively to the close relationship which indubitably exists between Corporeal and Mental states, have thought that *all* the operations of the mind are but manifestations or expressions of material changes in the Brain; that thus man is but a *thinking machine*, his conduct being entirely determined by his original constitution, modified by subsequent conditions over which he has no control, and his fancied power of self-direction being altogether a delusion; and hence that notions of *duty* and *responsibility* have no real foundation. Man's character being formed *for* him, and not

by him, and his mode of action in each individual case being simply the consequence of the reaction of his brain upon the impressions which called it into play. On this creed, what is commonly termed Criminality is but one form of Insanity, and ought to be treated as such. Insanity itself is nothing else than a disordered action of the Brain, and the highest elevation of man's psychical nature is to be attained by due attention to all the conditions which favour his physical development. \* \* \* \* It seems to the Writer that every system of Philosophy which regards the succession of mental phenomena as determined *solely* by the ordinary laws of physical causation, and which rejects the *self-determining* power of the Will (or, which is the same thing, regards the Will as only another expression for the *preponderance of motives*, or as the *general resultant* of the action of the Physiological Mechanism), virtually leads to the same conclusions." (P. 3.)

Dr. Carpenter therefore rejects these doctrines, and maintains that every man, independently of his brain, does "*really possess a self-determining power* which can rise above all the promptings of suggestion, and can, *within certain limits* (§ 25), mould external circumstances to its own requirements." It is obvious that the exact meaning to be attached to the terms here used should have been more clearly stated to render the doctrine intelligible as a part of physiology, and in relation to the chemical, physical, and vital laws which regulate brain-functions. But, in truth, it is a metaphysical dogma which Dr. Carpenter propounds, by which he believes he can reconcile the most antagonistic systems, such as, in theology, those of Manning and Martineau, and in metaphysics, of Stuart Mill and Hamilton. He may for like reasons expect, in physics, that his dogma will be found to reconcile Cox and Crookes with Carpenter, who will find in it a basis for their hypothesis of a "psychic force."

It is obvious that Dr. Carpenter has herein endeavoured to follow a twofold method, namely, the positive method and that of the old scholastic philosophy which separates the laws of life from those of thought, and classes them under "materialism." By that method the hypotheses of a self-determining power, "immaterial" yet not vital, is easily reached. This we shall subsequently consider; at present we will examine Dr. Carpenter's method of applying his hypothesis to mental physiology, and more especially to "automatic activity of the mind," as well as to the "physiological mechanism" and the automatic activity of the brain. Collating passages, it is found that a due cerebral development, judiciously attained by education, is the foundation of this self-determining power.



"8. This combination of the two distinct agencies in the mental constitution of each individual is recognised in the whole theory and practice of Education. For whilst, in its earlier stages, the Educator aims to call forth and train the Intellectual Faculties of his Pupil, and to form his Moral Character, by bringing appropriate *external* influences to bear upon him—and every one who really understands his profession will make it his special object to foster the development, and to promote the right exercise of that *internal* power by the exertion of which each individual becomes the director of his own conduct, and so far the arbiter of his own destinies—this power is exercised by the Will in virtue of its domination over the *automatic* operations of the Mind as well as the *automatic* movements of the body (§ 14); the real self-formation of the Ego commencing with his consciousness of the ability to determine his own course of thought and action." (P. 8.)

Examining this passage we learn that, in Dr. Carpenter's opinion, every man possesses a self-determining power, so that "within certain limits he can choose between what he thinks good and evil, right and wrong, just and unjust." Substitute, for this, that every man is capable of choosing within the limits of his cerebral development and healthy activity, the proposition is true, but we defer to the conclusion that there is a power termed the will which dominates over the cerebral functions independently of them. Again: we do not understand what is meant by the "automatic operations of the *mind*" as distinct from those of the *brain*; nor do we see any difference between the "ego," represented to possess the power of "self-formation," and the "individual." The word mind is frequently used as correlative with "cerebrum," and yet it is also used to represent something, in the sense as used in speculative mental science. A paragraph is subjoined.

"But *Mind*, like *Force*, is essentially *active*; all its states are states of *change*, and of these changes we become directly or immediately conscious by our own experience of them. In fact every term—as Sensation, Perception, Idea, Emotion—which expresses a Mental state is a designation of a phase of mental existence that intervenes between other phases in the *continual succession* of which our idea of Mind consists, and consciousness itself is nothing else than the designation which we give to the condition which is common to all these forms of activity." (P. 12.)

The general fact of mental physiology is this: that with the continual succession of the states of consciousness comprised under the term mind, there is a correlative continuous succession of encephalic changes which, as our knowledge stands at present, consist in molecular changes. The state termed

mental existence is thus a state of being or of living the *qua* states of the brain, upon the functional activity of which organ the mental states depend. But Dr. Carpenter uses his abstract terms in the metaphysical way, as if no such necessary dependence existed. That the will, meaning thereby the state of the man energising in a certain way, can control the course of thoughts, although those thoughts and their correlative brain states run on automatically, is as certain as that it can control movements of limbs, although these two are automatic: all this is admittedly true in fact.

Further, according to Dr. Carpenter's views, "the physiological mechanism," meaning the brain by that phrase, is also in a sense self-regulative as well as the mind and the will. [It is right that the reader should be aware the capitals and italics in the subjoined and other extracts are in the original.]

"The physiological mechanism has this peculiarity--that it *forms itself*, according to the mode in which it is *habitually* exercised, and thus not only its *automatic* but even its *unconscious* action comes to be indirectly modified by the controlling power of the Will, § 95." (P. 15.)

This self-formation of the physiological mechanism is, as is indicated by other passages (one of which we subjoin), that cerebral process by which knowledge is acquired, and the training of the limbs and organs of speech and sense, and the mental faculties perfected. The production of musical tones or of speech is explained in that sense. The professed anatomist, Dr. Carpenter observes, would have difficulty in stating what muscles are combined in action to produce a given musical tone or a syllable, "yet we simply conceive the tone or the syllable that we wish to utter, and say to our automatic self, 'Do this,' and the well-trained automaton does it." All this, although metaphorical, is intelligible from the physiological point of view. The development of organic memory, whether as to habits or modes of thought, leads up to a condition in which an individual can conceive a wish, or can be prompted by others to perform a certain volition; but then the whole cerebral condition of the moment is the "physiological resultant" of precedent vital activities. When, however, Dr. Carpenter introduces his "self-determining power," and substitutes that as will, for the individual, all becomes confusion.

"19. But we may go a step further, and assert that it may now be regarded as a well-established Physiological fact, that even in the most purely Volitional movements—those which are prompted by a *distinct purposive* effort—the Will does not directly produce the result, but plays, as it were, upon the automatic apparatus [a notion



of Johann Müller] by which the requisite organic Nervo-muscular combination is brought into action." (P. 20.)

Dr. Carpenter in all this ignores a most fundamental law of life, a knowledge of which is essential to the solution of the problems he discusses. The living brain, with which the psychologist has to deal, "forms itself" according to certain laws of life as certainly as a crystal "forms itself" according to certain laws of physics. But there is this difference, that the laws of life are essentially evolutionary, so that the energising at any moment to attain a desired end which is termed an act of will, whatever may be desired, or purposed, or included under the abstract terms, can only occur in accordance with the evolutionary condition, to which the "cerebrum" had either attained, or under which it was potentially active at the moment of willing. Of the great laws of evolution as applied to mental science the scholastic method takes no account. In repudiating them Dr. Carpenter is undoubtedly orthodox, but then he has also endeavoured to put the new wine of positive mental science into the old bottles of speculative philosophy, and the usual result follows.

In developing the doctrine we have criticised in relation to the reflex and automatic activity of the brain (or of the mind, for the two words are constantly used synonymously), Dr. Carpenter has to make various assumptions which we thought had been long exploded. In particular, he seems to hold that animals differ from man in being a sort of thinking machines, with no volitional power or choice. He also holds that the "reflex action of the cerebrum" excludes, or at least is independent of, the will in them.

"94. Whilst, however, we fully recognise the possession, by many of the lower Animals, of an Intelligence comparable (up to a certain point) with that of Man, we find no evidence that any of them have a volitional power of *directing* their Mental operations at all similar to his. These operations, indeed, seem to be of very much the same character as those we perform in Reverie or connected dreams; different 'trains of thought' commencing as they are suggested, and proceeding according to the laws of Association until some other disturb them. So long, in fact, as the current of thought and feeling flow on under the sole guidance of suggestion, and without any interference from the Will, it may be considered as the expression of the *reflex action of the Cerebrum*, called forth, like that of other nerve-centres, by the stimulus conveyed to it from without, the seat of that activity being its expanded layer of cortical substance. (The extension of the doctrine of reflex action of the Brain was first advocated by Dr. Laycock, in a very important essay read before the British Association in 1844, and published in the 'British and Foreign Medical Review,' for January, 1845.)

"This reflex action manifests itself not only in Psychical change, but also in Muscular movements; and these may either proceed from simple ideas without any excitement of Feeling, in which case they may be designated *ideo-motor*; whilst, if they are prompted by a Passion or Emotion, they are known as *emotional*. The nature of the response made by the reflex action of the Cerebrum will depend upon the condition of the organ at the time when it receives the impression, and that condition among the lower animals may be regarded as the *resultant* in each individual of the modifications which its inherited constitution has undergone from the influence of external circumstances:

"95. But whilst the Cerebrum of Man, in common with that of the lower Animals, has a reflex activity of its own, which, in the first instance, may be regarded as the direct resultant of his congenital Constitution modified by early training, an additional and most important influence subsequently comes into play, namely, the directing and controlling power of the Ego's own *will*, in virtue of which he can to a great degree *direct* his thoughts and *control* his feelings, and can thus rise superior to circumstances," &c. (p. 105).

In all this the old notion that man is not only superior to all other animals, but wholly different from them cerebrally, is assumed. In doing this the great law of evolutionary continuity of structure and function in animal life is equally ignored. As a fact of positive science, there is no foundation for the assumption that the lower animals live mentally in what is in man a dream or reverie.

Concurrently with this reproduction of the Cartesian doctrine as to animal life, the old and correlative hypothesis of a *sensorium commune* is reproduced:

"98. . . . But it appears to the writer that this influence [of the will on the course of strictly mental operations] is by no means so direct as is commonly supposed, and that observation of our own Psychical phenomena entirely justifies the belief which physiological considerations tend to establish—that the operations of the *cerebrum* are in themselves as automatic as are those of other nerve-centres, and that the volitional control which we [the concrete Ego and not the will] exercise over our thoughts, feelings, and actions, operates through the *selective attention* which we [not the will] determinately bestow upon *certain* of the impressions made upon the Sensorium out of the *entire aggregate* brought thither by the 'nerves of the internal senses.' (§ 89.)

"In this point of view it is the *Sensorium*, not the *cerebrum*, with which the Will ["the control *we* exercise"] is in most direct relation; and in order that this doctrine, which lies at the basis of the whole inquiry, as to the relation of the Will to the motives, and the mode in which it determines our character and actions, may be rightly apprehended, it is necessary here to consider the following



Physiological question : whether *Cerebral changes are in themselves attended with consciousness, or whether we only become conscious of Cerebral changes as states of ideation, emotion, &c., through the instrumentality of the Sensorium*—that is, of that aggregate of sense-ganglia through the instrumentality of which we become conscious of external sense-impressions, and thus feel sensations." (P. 108.)

The phrase "nerves of the internal senses" was first used, Dr. Carpenter remarks, by "a sagacious old anatomist, Reil," to denote the fibres which connect the thalami and corpora striata with the hemispheres. These centres, conjoined with the ganglia of the olfactory, optic, and auditory nerves constitute Dr. Carpenter's *sensorium commune*, of which a diagram is given (p. 97). The "selective attention" referred to in the preceding extract is that volitional faculty usually termed attention :

"But the power of the Will is exerted in the *purposive selection* from among those objects of consciousness which Sensations from without and the working of the internal 'Mechanism of Thought and Feeling' bring before the Ego (whether simultaneously or successively), of that which shall be determinately followed out ; and in the *intensification of the force of the impression* which seems the direct consequence of such limitation. This state is what is termed *Attention*, in regard to which it was well said by Sir William Hamilton, that its *intensity* is in a precisely inverse ratio to its *extensity*, and it will be the writer's object to show that it is solely by the volitional *direction of the attention* that the Will exerts its domination." (P. 25.)

Throughout these various extracts there is the same error in method. Attention is a name for a number of highly complex physiological acts, having their own excitor nerves, nerve-centres, and mechanical apparatus, but nowhere is there an analysis of these or their mechanical relations indicated, even hypothetically. One experiment is referred to in which the cerebellum seemed to have a control over the movements of the eyes, those important organs in acts of attention, but no definite use is made of the fact. The terms attention, volition, suggestion, sensation, consciousness, the will, ego, mind, mental, are all used, sometimes as abstract terms, sometimes to indicate causes, sometimes effects and in some instances a series of conditions of brain-tissue. Hence numerous repetitions, explanations, and confusions.

Reverting to the fact of positive science, that every state of consciousness whatever has its corresponding *locus in quo*, and is only manifested under fitting conditions of that *locus*, let us now inquire how man's intellectual and moral freedom is con-

nected with brain-function. Dr. Carpenter freely acknowledges that the continuous use of stimulants such as opium and haschisch "has a manifest tendency to produce permanent weakening of the volitional power" (p. 382), and to induce "almost complete paralysis of the Will" (p. 647). If we speak of palsy or weakening of the nerves, motor or sensory, or of the nerve-centres, the phrases are intelligible, and if we were to say that in the cases of De Quincey, Hartley Coleridge, and many drunkards and moral imbeciles, that part of the brain which is subservient to volitional energy and moral notions was injured, and so paralysed by the use of drugs, we should have a solid foundation in fact; and if to that portion of the encephalon we apply the laws of reflex action we have an equally solid starting-point for the anatomy, physiology, and philosophy of the will, whether as its phenomena are manifested in health or in disease, in good or in evil. But this method, if adopted by Dr. Carpenter, would exclude the widely divergent doctrine as to the will of speculative philosophy, and would of necessity include the two great laws of biological philosophy, viz. evolution and reversion, and which Dr. Carpenter of necessity rejects. Yet with the aid of these he could have brought all the phenomena of the will, and the facts of man's intellectual and moral freedom in their relations to the organism, within the scope of a physiology having a solid basis in biology. To this end all that he needed was to extend the doctrine of reflex action evolutionally to the highest and latest development of the cerebrum or hemispheres, according to the method laid down and followed by Dr. Laycock, both in the essay to which Dr. Carpenter refers and in various other works. It is, in short, the essential characteristic of that doctrine and method to include the laws of evolution and reversion, commencing with the evolution and reversion as manifested in molecular energies and chemical compounds. As to reversion of cerebral functions, there is a full demonstration in the appendix to the essay.

As to evolution of the brain and nervous system, we take the following paragraph from Dr. Laycock's essay:

"Four years have elapsed since I published my opinion, supported by such arguments as I could then state, that the brain, although the organ of consciousness, was subject to the law of reflex action, and that in this respect it did not differ from the other ganglia of the nervous system. I was led to this opinion by the general principle that the ganglia within the cranium, being a continuation of the spinal cord, must necessarily be regulated as to their reaction on external agencies by laws identical with those which govern the functions of the spinal ganglia and their analogues in the lower animals; and I was confirmed in this opinion by finding, after the



investigation and collection of known facts, that observations and arguments like these satisfactorily adduced in proof of the existence of the reflex function of the spinal ganglia, may be brought forward in proof that the cerebral ganglia have similar endowments. In the present paper I purpose to give these proofs connectedly.”<sup>1</sup>

Here, then, a theory is stated, and the method of verification laid down. With this latter object in view, Dr. Laycock selects the phenomena of the hydrophobic gasp in the first instance to show that Dr. Marshall Hall’s limitation of the reflex function to the spinal nerves as excitors, and to the tubercular quadrigemina and all below as reflex centres, was inadmissible. He remarks as follows:

“The acknowledged excito-motor phenomena of hydrophobia may be induced—firstly, through the sensual nerves of touch, as by the contact of water with the surface of the head, hands, chest, the lips, and pharynx; second, by a current of air impinging on the face or chest. These causes act undoubtedly on the [class of] nerves mentioned. But, thirdly, a bright surface, as a mirror; fourthly, the sight of water; or, fifthly, the sound of water dropping; or, sixthly, the idea of water, as when it is suggested to the patient that he shall drink; all most indubitably induce excito-motory phenomena as decided and distinct as the first and second causes.”

Dr. Laycock then gives cases in illustration, and also refers to the reflex action of colours, especially of red, and of odours.

In stating the conditions of reflex action in relation to consciousness, Dr. Laycock distinctly points out that it is not the sensation of tickling, or the feeling of pain, or other states of consciousness which induce the reflex acts, but changes in the mechanism upon which both the sensation or other conscious states and the motor acts depend. Hence in the hemispherical ganglia (as Dr. Laycock designates what Dr. Carpenter names the “cerebrum”), just as in the ganglia of the spinal cord, there are both motor and sensory tracts of vesicular neurine, which, to distinguish them from the merely motor and sensory tracts, and as being in special relation to mental states, he designates *ideagenic* and *kinetic substrata* in an appendix to the essay. It is especially in articulate speech, and in instinctive acts, and acquired habits, that these organic conditions of mind are most cognisable. Referring to a class of cases now included under *aphasia*, and conjoined with hemiplegia, Dr. Laycock thus indicates the kinetic basis of language or speech.

“In these cases when the will is directed to the enunciation of one word, as ‘bread,’ the individual utters another word, as ‘boots.’

<sup>1</sup> “On the Reflex Function of the Brain,” read at York before the Medical Section of the British Association for the Advancement of Science, 28th Sept., 1844, ‘Brit. and For. Med. Rev.,’ Jan., 1845.

Sometimes it is a letter only that is thus pronounced, as *z* for *p*; sometimes the words of foreign languages are mixed up in confusion, in spite of the individual's efforts to articulate right. *This phenomenon is analogous to the irregular acts of groups of muscle.*" (Op. cit., p. 306.)

In short, these phenomena constitute the chorea of language.

The evolutionary nature of these kinetic bases or substrata is further shown by a case of rhythmical chorea in which ideas of *space* and *time* were gradually evolved as the diseases extended upwards from the spinal centres to the hemispheres. Dr. Laycock indicates in his comments on these cases both the reflex nature of irresistible and unresisted impulses and the inhibitory functions of the hemispheres.

In the paper on "Certain Organic Defects of Memory," after a lapse of thirty years, Dr. Laycock supplies incidentally other illustrations of his views, more especially as to the reflex character of various phenomena in *aphasia*.

Thus, for example, there are aphasiacs who can read, although mute as to the expression of their thoughts; others who can speak their thoughts, yet cannot write, and *vice versa*. Dr. Laycock points out that these differences are due to differences in reflex action of differing kinetic substrata. These latter are trophic reflex results formed under the operation of their appropriate external or excitor impressions received by the hemispherical neurine along the sensory nerves, which in speech are chiefly auditory and visual. In these respects language is to be classed with the instincts and instinctive acts:

"The acquisition of language is in precisely the same order as the acquisition of power and habit of walking. In both cases external impressions are needed to develop the motor and sensory synesis upon which speech, use of the hands, and direction of the limbs, and balancing of the body in locomotion depend. None of us remember how painfully we acquired the facile uses of our legs; but if the sensory impressions upon which they depend are no longer transmitted from the plantar surfaces in walking, as occurs in locomotor ataxy, the individual must trust to visual impressions; and failing these, as when the eyes are shut, he can no longer resist gravity, and so maintain his equilibrium. So also if a person become deaf, he generally becomes defective in his speech; if born deaf, he is mute. If suffering from aphasia, he may be able to read when he cannot express his thoughts, because the visual excitor impressions from the printed or written page reproduce the synesis, although those of audition, or from above and connected with association of ideas being cut off, he is speechless. I have repeatedly shown this difference in the reflex phenomena of aphasia to my clinical class, as to both



audition and vision ; and I conclude that to understand its pathology we must thus differentiate the various sources and centres of cerebral reflex action into at least motor and sensory." ('*Edin. Med. Journ.*,' April, 1874.)

The researches of Hitzig and Fritsch, the first series of which appeared in Du Bois Reymond's '*Archiv*' for 1870, Heft 3, are confirmative of these conclusions, drawn by Dr. Laycock, from clinical facts. They establish as the result of experimental investigations into "the electrical excitability" of the brain that part of the hemispheres of the brain is motor, *i. e.* reacts to electrical stimulus, so that various movements of the limbs of the animal follow ; whereas another part is non-motor, and is not, therefore, in at least direct motor relation. The former may be held to correspond with the kinetic substrata of Dr. Laycock, the latter with the ideagenic.

The motor or kinetic are situate towards the front of the cerebral convexity, the non-motor posteriorly.

The relations of the will to the hemispherical ganglia through the two classes of substrata are also shown by Dr. Laycock, without any need for a hypothesis of a self-determining power playing upon the fabric or mechanism of thought. The following points out how complex is the state of consciousness termed the will, and how varied its organic relations to memory.

"That the seats of the motor and sensory [memory] are distinct may, I think, be learnt by an observation which, however, each reader must make upon himself. Having selected some song, of which both the air and the words have been stored up in his brains, the reader should first try to sing both the air and the words mentally ; that is to say, without any motor activity whatever, and he will find that he can do this, unless it be that, perchance, his breathing keeps the time of the air involuntarily. Having done this sufficiently often, let him next end the process by energising into actual singing. In this way he cannot fail to discover that he has been engaged in two distinct cerebral processes, the one restricted to consciousness, thought, or mind ; the other combining motor activity with the mental states." (*Op. cit.*)

Here, then, are at least two distinct kinds of volition, having reference to two distinct organic bases. In the one, what Dr. Carpenter calls the attention is directed to a non-motor basis ; in the other, to one in or through which energy is evolved to be directed to appropriate muscles, in exactly the same order as in spinal reflex action. So that there are two distinct kinds of will, the ideational or sensorial, and the volitional proper or motorial.

That the latter kind is capable of further organic analysis is shown by the motor phenomena of emotions or the emotional

will, not only in merely instinctive acts, but in speech. In dreams, with vivid emotional volition, and endeavours to escape from danger, there is commonly such palsy that the dreamer cannot move a limb, and even cries are weak or inarticulate. This is due probably to the same condition of the hemispheres by which memory as retention (or synesis) is hindered.

But the will of a given moment in any form is the result of evolution of energy, and the so-called material bases is strictly dynamical or vital. In the paper referred to the evolutionary nature of the latter is explicitly stated as a fundamental element of the doctrine of the reflex function of the brain.

"In truth, these conditions, although material, since matter underlies them, are not material in any scientific sense of the term, but vital. The material basis has, in fact, evolutionary properties as manifestly as the primordial cell of the seeds and ova of plants and animals. I have, therefore, designated them *substrata*, those which belong to states of consciousness being *ideagenic*, and those subservient to volition and muscular action *kinetic*. The evolutionary properties of these substrata are best indicated by the phrase *ideagenic*, inasmuch as new ideas the result of new acquisitions of knowledge tend to evolve and develop as certainly as a seed or an egg, so that they are in no wise merely material. I have repeatedly indicated these characteristics [references given]. It is absolutely essential to practical ends that the true nature of the organic basis of memory should be understood to be something more than a physical mechanism, like a muscle, or a merely mechanical arrangement of cells and fibrils, the members of which may be corrected and reckoned as so many 'acquisitions.' This is an old anatomical and physiological doctrine, taught by Hartley, Bonnet, Hooke, Haller, and others, by Akenside poetically, and which has been recently revived by Professor Bain." (Op. cit.)

We will now endeavour to appreciate Dr. Carpenter's anatomy in relation to function. A diagram (p. 125) indicates the supposed anatomical course through the "sensorium" upwards of impressions made on the spinal and other sensory nerves. Here, however, as elsewhere, the use of abstract terms obscures the meaning and leads to error.

"109. In that action and reaction, however, between the mind [brain] and all that is outside it, in which the Conscious Life of every Human *Ego* [thinking man] consists, the whole Cerebro-spinal system participates. For, in virtue of the peculiar arrangement of the nervous apparatus, every excitor-*impression* travels in the *upward* direction, if it meet with no interruption, until it reaches the Cerebrum [cortical or grey matter of the hemispheres] without exciting any reflex movements in its course. When it arrives at the sensorium it makes an impression on the consciousness



[sensory ganglia] of the individual, and thus gives rise to a *sensation*; and the change there induced, being propagated onwards to the Cerebrum, becomes the occasion of further changes in its cortical substance, the downward reflexion of whose results to the Sensorium gives rise to the formation of an *idea*. If with this idea any pleasurable or painful *feeling* should be associated, it assumes the character of an *emotion*, and, either as a simple or as an emotional idea, it becomes the stimulus to further Cerebral changes, which, when we become conscious of them, we call *Intellectual operations* [perception, thought, memory, judgment, will]. These may express themselves [be expressed] either directly in respondent movements, or *indirectly*, by supplying motives to the Will, which may exert itself [be exerted] either in checking Muscular movements or in controlling or directing the current of Thought and Feeling." (P. 123.)

Let us compare this ideal account of the anatomical order of events with realities. A man has hay asthma with fits of violent sneezing. The spasmodic breathing and the sneezing are alike "true spinal" excited acts, due either to centric or to peripheral irritation, but conjoined also with a sensation of suffocation and of tickling, and a feeling termed a desire to sneeze, which it is agreeable to gratify. Is the impression in this series of events stopped in its course upwards, so that it may cause these excito-motor and excitor-sensory phenomena? No; on the contrary, it extends upwards to the cerebrum or hemispheres, where it is expended in the excitation of the "intellectual operations" known as thought and will, when a desire to energise to a beneficial end arises just as the desire to sneeze is a desire to energise to an agreeable and beneficial end. In all this the reflex action from the hemisphere downwards is surplusage, as well as contrary to the order of events in spinal reflex action.

Further, while the hemispheres and not the abstract will energise positively, they also energise negatively, so as to inhibit, control, or prevent reflex acts, as Dr. Carpenter shows.

"Thus we are ordinarily unconscious of that internal need for air by which our movements of respiration are prompted; and it is only when we have refrained from breathing for a few seconds that we experience a sensation of uneasiness which impels us to make forcible efforts for its relief. Notwithstanding, however, that the Cerebrum is unconcerned in the ordinary performance of those automatic movements, yet it can exert a certain degree of control over many of them, so as even to suspend them for a time," &c. (P. 122.)

The inhibitory functions of the vagus and of other nerve-centres is here represented evolutionally as a function of the

hemispheres, or metaphysically as the restraining will, so important in monads.

It will be observed that the term reflex is used in two distinct senses in the passage quoted, in which it is stated that an "impression" will travel upwards without exciting any reflex movements in its course, provided it meets with no interruption. Dr. Carpenter, therefore, devotes a paragraph (too long to quote) to show how "excito-motor," "sensori-motor," and "ideo-motor" phenomena are induced, according as the impression is interrupted in the cord, the sensorium commune and the cerebrum respectively. This occurs in diseased states, but it has already been shown that there is no need in the healthy state for any such hypothetical interruption. The differences in the reflex results of impressions, whether made on the eye or the ear, or other sense apparatus, will depend just as much on the kind of excitor impression as on the kind of special tissue in which the molecular process that constitutes reflexion takes place. But in any case the manifestation of the mental phenomena, whether considered as excito-sensory or excito-motor, will depend upon the condition of the cerebrum at the moment of activity. If the portion of the hemispheres in which the energising to attain a desired end termed will takes place be palsied, or so conditioned in any way that its function is internal or modified, then the kind of will corresponding to that portion will fail or be modified, whether it be moral or intellectual, or motor as to limbs, or vocal or graphic.

This, in brief, is the whole organic theory of weakness of will and of judgment, of moral insanity and imbecility, and of insane criminality, whether it be chronic or suddenly impulsive, when it is closely analogous to the temporary moral paresis of immoral dreaming. Reflex action therefore may be either normal or abnormal, and includes all kinds of cerebral function. The processes are equally alike, as Dr. Laycock observes in the Appendix to his Essay.

"Researches of this kind, whether instituted on the insane, the somnambulist, the dreamer, or the delirious, must be conducted like researches in analytical chemistry. The reagent is the impression on the brain; the molecular changes following the application of the reagent are made known to us as ideas. In chemical analysis we know the molecular changes only by the change in form, refractive powers, and other circumstances, induced by the reagent; in cerebral analysis we *feel* the change, or observe its results on the efferent nerves. It is very probable that only on researches of this kind can a scientific spiritualism be established, and through them the link seized that connects the spiritual with the material world."



And these remarks, we may add, are made in reference to the case of a patient "who seems utterly will-less. He expresses his ideas as they spontaneously arise in associated sequence, the combinations being singularly varied, but traceable to a common root or centre of impulse." Hence it follows that cerebral reflex action, as originally expounded, is excito-sensory, and not merely motor, nor is it due to interruption of the course of impressions. Reflex action, in short, is the general law of reaction of any living tissue, whatever may be its special endowments; the cause or so-called impression being a vibration or other mode of molecular energy which disturbs the equilibrium of the molecules of the tissue on which it impinges.

Dr. Carpenter applies the principle, in a crude form, to the excito-sensory phenomena of the brain, and thereby elucidates with great variety of illustration the organic character of the phenomena of mind. Anatomically, however, he limits his diagrammatic illustration to reflex motor phenomena, and mainly to those of the limbs, as thus: "excito-motor" are due to reflex action of spinal centres, "sensori-motor" to that of the sensorium commune, while "ideo-motor" (more properly cerebro-motor) arise from that of the hemispheres. But he has a theory of "unconscious cerebration," for which he claims priority in connection with his theory of a sensorium commune.

Dr. Carpenter thus writes as to this process:

"416. Having thus found reason to conclude that a large part of our Intellectual activity—whether it consists in Reasoning processes or in the exercise of the Imagination—is essentially *automatic*, and may be described in Physiological language as the *reflex action of the Cerebrum*, we have next to consider whether this action may not take place unconsciously. To affirm that the Cerebrum may act [react?] upon impressions transmitted to it, and may elaborate Intellectual results, such as we might have attained by the intentional directions of our minds to the subject, *without any consciousness* on our own part, is held by many Metaphysicians, more especially in Britain, to be an altogether untenable and even a most objectionable doctrine. But this affirmation is only the Physiological expression of a doctrine which has been current among the Metaphysicians of Germany from the time of Liebnitz to the present date, and which was systematically expounded by Sir William Hamilton—that the Mind may undergo modifications, sometimes of very considerable importance, without being itself conscious of the process, until its results present themselves to the consciousness in the new ideas, or new combinations of ideas, which the process has evolved. This 'unconscious Cerebration,' or 'Latent Mental modification,' is the precise parallel in the higher sphere of Cerebral or Mental activity to the movements of our limbs, and to the direction of these movements through our visual senses, which we *put in train* volition-

ally when we set out on our habitually repeated walk, but which then proceed not only *automatically* but *unconsciously* so long as our attention continues to be uninterruptedly diverted from them; and it was by reflection on this parallelism, and on the peculiar structural relation of the Cerebrum to the Ganglionic tract, which seems to constitute the *Sensorium* or centre of consciousness, alike for the *external* and for the *internal* senses (§ 100), that the writer was led . . . . to the idea that cerebral changes may take place *unconsciously*, if the *Sensorium* be either in a state of absolute torpor or be for a time non-receptive as regards those changes, its activity being exerted in some other direction; or, to express the same fact psychologically, that motor (?) changes, of whose results *we* subsequently became conscious, may go on *below the plane* of consciousness, either during profound sleep or while the attention is wholly engrossed by some entirely different train of thought." (p. 515.)

In a footnote, Dr. Carpenter states that subsequently to the first publication of his views on this subject (in the fourth edition of his 'Human Physiology,' 1852), he had a communication from Dr. Laycock as to the claim of priority. This is an error of date, As may be seen by reference, Dr. Carpenter made no claim in that edition to the doctrine of "unconscious cerebration," but in a footnote (p. 799) recognised his obligations to Dr. Laycock. It was subsequently to the edition of 1855, from which that footnote was omitted, and when "unconscious cerebration" was first mentioned (see Preface) as peculiarly Dr. Carpenter's, that Dr. Laycock reclaimed, and this for obvious reasons. Once the doctrine of reflex cerebral action admitted, that of "unconscious cerebration" was logically included. Dr. Carpenter clearly puts this obvious conclusion thus :

"Looking, therefore, at all the automatic operations of the mind in the light of 'reflex actions' of the Cerebrum, there is no more difficulty in comprehending that such reflex actions may proceed without our cognisance—their results being evolved as *intellectual products* when we become conscious of the impressions transmitted along 'the nerves of the internal senses' from the Cerebrum to the *Sensorium*—than there is in understanding that impressions may excite muscular movements through the 'reflex' power of the spinal Cord without the necessary intervention of sensation." (§ 68.)

In short, "unconscious cerebration" is only another phrase for automatic activity of the hemispheres, going on independently of sensation or consciousness as a proximate or exciting cause, according to the law of spinal reflex action. Dr. Carpenter says Dr. Laycock made no distinct statement to the effect that such reflex cerebral action might be unconscious in this sense, and that all the illustrative examples he brought forward were of a kind in which consciousness was involved. This is a misappre-



hension on the part of Dr. Carpenter, due, perhaps, partly to the circumstance that he uses the word consciousness in varying senses, partly to the non-perusal of Dr. Laycock's writings published previously and subsequently to the date of the essay (1844) to which he refers, and which date from 1838.<sup>1</sup> The earliest essays were republished in an enlarged and systematic form in 1840, as a 'Treatise on the Nervous Diseases of Women,' in which the doctrine of reflex cerebral action and its practical applications to pathology are elucidated. Dr. Carpenter can refer more especially to the chapters of that treatise headed respectively "Some Points in the Metaphysics and Physiology of the Nervous System considered;" "The Brain and Sensitive Nerves as the Organs of Consciousness;" "The Instinctive Actions in relation to Consciousness—the Brain subject to the Law of Reflex Action;" "The Action of the Will and of Internal and External Stimuli on the Hemispherical Ganglia;" "The Pathological and Physiological Relations of the Nervous System to the Ovaries," &c.

These titles sufficiently indicate the unity of doctrine and method which Dr. Laycock adopts, and upon which his systematic work is constructed ('Mind and Brain; or, the *Correlations of Consciousness and Organization*,' first edition, 1860; second edition, 1869). In the correspondence of Dr. Laycock with the late Professor Reid and George Combe ('Lancet,' vol. i, 1845), in which Reid advocated views closely like those of Dr. Carpenter, Dr. Laycock, emphatically maintained that consciousness, whether as sensation, emotion, or volition, was neither the immediate cause nor a necessary condition of cerebral action. It was not, indeed, until 1852 that Dr. Carpenter realised the true significance of Dr. Laycock's views, directly opposed, as they were, to the current doctrines of all the leading physiologists and metaphysicians of the day, including Dr. Carpenter himself.

If Dr. Carpenter would take the trouble to read those writings carefully, or even the essay to which he refers, he will find that, according to Dr. Laycock's views, reflex cerebral, like reflex spinal activities, go on "*mens conscia vel inscia*," to use Prochaska's phrase as to the latter, who, by-the-bye, fixed, like Dr. Carpenter, upon the sensorium commune as the place of reflexion. Consciousness plays no necessary part in cerebral activity as a cause, nor is it a necessary accompaniment, except when the brain is so altered that the conditions which render conscious states impossible also render it incapable of reflex sensory activity.

<sup>1</sup> "Analytical Essays on Irregular and Anomalous Forms of Hysteria," 'Edin. Med. and Surg. Journ.,' July, 1838; Oct., 1838; July, 1839.

In truth, it is as to the *anatomical explanations* of the phenomena by means of his hypothetical sensorium commune that Dr. Carpenter can claim novelty of views; but even in these explanations, as in others, the vague use of abstract terms has confused his perceptions of the relations of consciousness to cerebral structures and activities. The term "cerebration" was coined, as a counterpart to "respiration," "digestion," &c., by an ardent phrenologist named Engledue, to denote mental activity as a function of, and in inseparable relation to, the cerebrum, and independently of any "immaterial" cause. Taking it as denoting cerebral activity in relation to consciousness, what meaning does Dr. Carpenter attach to the latter term? Chiefly in the sense of the speculative school. For example, in his illustration of automatic walking to a predetermined point (see above), the word is used in the sense of *knowing*. Thus Sir William Hamilton says we cannot be conscious and not *know* that we are conscious, which is correct in his use of the word, not correct according to the physiological use; for a man may feel pain, and yet not know that he feels or has felt, for to *know* that *he* feels he must have a knowledge of his "ego," to use a favourite word of Dr. Carpenter's. Hence men in delirium will groan, and thus express that they feel pain, and yet will attribute the groans and sufferings to another person. So, again, memory and the abstract idea of self are necessary to that notion of personality which underlies the consciousness of the meta-physician in the sense of knowing.

The physiological question is otherwise. Can automatic cerebral activities go on when an individual is wholly unconscious, or, in other words, when the encephalon is in that state that it either cannot receive impressions, or else, receiving, cannot so react upon them that a conscious state follows? To answer this question it is necessary, in the first instance, to determine upon what physical conditions the brain-states which coincide with conscious states depend. Now, it is quite certain that too low a temperature of the brain will destroy this capability; so also various chemical and mechanical conditions. But in all the examples of "unconscious cerebration" adduced by Dr. Carpenter the individual is or was in a state of consciousness, albeit not of knowing consciousness. So also as to the "latent consciousness" of Hamilton. That great thinker maintained that consciousness never ceased, because (as he held) it depended on an immaterial something of the existence of which consciousness is the necessary manifestation; and in holding this he logically excluded from his inquiries both mental physiology and biology.

Further, Hamilton held, too, in common with all of his school, that consciousness is a cause; that sensations of all



kinds—pain, corporeal and mental—emotions of whatever kind they may be—and volitions and ideas, are the causes and not the results of cerebral conditions. Does Dr. Carpenter hold this doctrine? We cannot say; for though he affirms it *passim*, yet by using “mind” for “brain,” and *vice versa*, he both affirms and denies it. And this is what Hamilton, by a like use of terms, can also be made to do; “Consciousness cannot exist,” he observes, as quoted by Dr. Carpenter, “independently of mind [brain].”

J. S. Mill is more consistent, because he attributes the phenomena of unconscious cerebral activity (cerebration, as the late Sir John Forbes remarked, is a “vile word”) to unrecognised changes in the brain, which he considers to be the constant physical antecedents. To these, however, should be added the cerebral coincidents and sequences.

Passing from this subject to the general question of connection of body and mind, Dr. Carpenter, in all his discussions on this point, fails to notice the *reflex trophic action* of the hemispheres, whether exercised on the body and its organs or within and on the encephalon. To the former class of phenomena belong, not only the chemical and other influences exercised on the muscles during an act of motor volition, but also all those bodily states associated with the conscious states termed attention, imagination, suggestion, and the emotions in general. In all these a reflex trophic action of the cerebral hemispheres on the body is manifested in the form of various changes in nutrition, secretion, innervation, as certainly as in muscular action and motion of limbs.

In like manner there are excito-trophic changes in the hemispheres themselves, and which constitute the basis of that part of organic memory corresponding to the record or *retentum* of metaphysicians, and which Dr. Laycock denotes by the term *synesis*.

It is only by thus extending the inquiry into the molecular phenomena and forces of living tissue in general that the obscure problem of the influence of the mind on the body, and conversely, can be solidly elucidated. In one of his earliest papers Dr. Laycock points out the evolution of the cerebral molecular energies through the lowest forms of organisms, and the connection of these latter with physical molecular energies, a principle since worked out so laboriously by Mr. Herbert Spencer.

“20. If we would obtain a large and definite knowledge of the action of force upon matter and intelligence in exciting the phenomena of life and thought as displayed in man, we must examine the laws of its action as exhibited both in every living organism and in the molecular changes of inorganic matter. A thousand circum-

stances assure us that between these last and the highest efforts of the human intellect there is a continuous chain of phenomena, although we have been unable to follow it link by link," &c.<sup>1</sup>

Again, as to the nature of the energies known as will, attention, suggestion, imagination, Dr. Laycock remarks—

"The only motions, now remaining, then, for which we have to seek an analogue in sensorial phenomena are those of irritability. To save the time of the reader I must here hypothetically assume that the brain is not only a centre, but a periphery, corresponding to the external periphery from which sensitive nerves originate, and in which the changes originated by [the forces of] matter commence, but itself originating fibrils which carry changes in the opposite direction; so that the action of the will on this internal periphery in exciting sensation and emotion must be considered analogous to that of force on the external periphery. The action of force and will, then, on organized matter, are identical, and the molecular changes induced by the will on the internal periphery are analogous to those induced by the force of matter on the external. Consequently these centric changes are analogous to those of irritability." ('Edin. Med. and Surg. Journal,' July, 1839, p. 19.)

These views are reproduced and more systematically developed in the treatises referred to.

The evolutionary trophic functions of the brain have yet to be investigated. Dr. Carpenter seems to have no idea that the convolutions have regulative functions equally important in relation to corporeal as to conscious states. Recent researches into cerebral pathology tend to the conclusion that the brain has a unifying trophic function in connection with and in relation to the cerebellum. He seems also to have profited little by the most recent inquiries into the function of that important central division of the encephalon, still holding that the direct communication which it has with both columns of the spinal cord and the comparatively slight connection it has with the higher portions of the brain, justify the conclusion that it is rather concerned in the regulation and co-ordination of muscular movements than in any proper psychical operations. Anatomical facts are, however, otherwise. The cerebellum has an intimate anatomical connection both with the hemispheres through the superior commissures and with every portion of the nervous system in the *pons Varolii* (the one undoubted centre of the nervous system), by means of the middle commissures or *crura*.

Although we have thus criticised freely in the interests of a more exact mental physiology what we think imperfect or erro-

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<sup>1</sup> 'Edin. Med. and Surgical Journ.,' July, 1839, p. 9; and 'Treatise on the Nervous Diseases of Women,' 1840, p. 92.



neous in Dr. Carpenter's views, we can recommend the work strongly to our readers as being full of interesting facts, of which those which have come within his personal experience are, perhaps, the most interesting. His theological philosophy of the will we shall criticise in our next number.

We have too little space left for a fitting notice of the works of Dr. Maudsley. In addition to the lectures printed in the first edition of 'The Body and Mind,' and two other essays, we have a reprint of two essays published in the 'Journal of Mental Science' on "Hamlet" and Emanuel Swedenborg. The great play of Shakespeare is reviewed with much philosophical acumen, and the hallucinations of the mystic have ample justice done them. In the essay on "Conscience and Organization," read to the British Medical Association, an important practical question in ethics and jurisprudence is discussed, and which is more fully developed in the work on 'Responsibility in Mental Disease,' written for the international series. In all these essays there is abundant evidence of a highly cultured and philosophical mind. Passages which would read well if detached from the context—a severe test of excellence—are numerous. How far much of what Dr. Maudsley advances is original is doubtful, because he leaves his readers in doubt whether the views he states are his own, or are simply those of other inquirers lucidly related or adapted to particular instances. As to some of these, however, there can be no doubt; they are evidently derived from unacknowledged sources.

The essay by Dr. Laycock on certain defects of organic memory has for its object the importance of distinguishing anatomically, both in science and in practice, between the two elements of memory noted from time immemorial by philosophers.

The fundamental organic change is the recording or conservative process in memory which, according to Dr. Laycock (as we have already observed), is a reflex trophic process of the hemispheres, excited, like reflex motor and reflex sensory changes, by appropriate incident excitator impressions. This process he terms *synesis*. If the excitator impressions be prevented in any way reaching the hemispheres, as when the senses are shut in sound sleep, there is no *synesis*. So also if the sensory centres through which the excitator impressions have to pass be defective there will be the same defect and a special kind of loss of memory results, which is to be distinguished from *amnesia* or loss of recollection by the term *asynesia*. Dr. Laycock adduces cases of shock to the occiput and the occipital region in which *asynesia* was the result, and which was relieved by counter-irritants over the occiput. He advocates the anatomical theory that the excito-

trophic impressions upon which synesis depends pass through the occipital and basilar regions of the hemispheres.

In another class of cases, however, the defective record or synesis is due to defective trophic power of the recording tissue. In these cases, more especially as to the motor synesis, there will be both defective record and reminiscence, such as is seen in senile and other forms of dementia.

The cerebellum has an important influence, according to Dr. Laycock's views, on the trophic energies of the hemispheres. It is through this great encephalic organ, having corporeal centres as distinct from the mental, that hereditary tendencies to corporeal strength or to weakness of brain-nutrition are derived; so that the cerebellum influences the hereditary tendency to insanity and cognate diseases. We have no space, however, to discuss these views.

We shall notice the other works on our list in our next number.

## II.—On Zymotics.

THE literary activity of the present day is fruitful not only in good work advancing science, but also in much destructive work whereby the accepted doctrines of past years are ruthlessly overturned. The work of destruction, indeed, is fascinating to some minds, and iconoclastic zealots are as readily brought to the surface when scientific demolition is the order of the day, as are destroyers of graven images when a crazy enthusiasm against false gods besets a people. In this matter of zymotics the destructive principle has been vehemently manifested, so that the results of the observation and clinical experience of past ages have been cast aside as worthless, and even as pernicious. Dr. Parkin remarks, in the introduction to his work—

“Painful it is to reflect that the evils arising from what it will

<sup>1</sup> 1. *Epidemiology; or, the Remote Cause of Epidemic Diseases in the Animal and in the Vegetable Creation.* By JOHN PARKIN, M.D., F.R.C.S. London, 1873. Part I.

2. *The Pathology and Treatment of Smallpox.* By ROBERT H. BAKEWELL, M.D. London, 1872.

3. *Smallpox: the Predisposing Conditions and their Preventives, with a Scientific Exposition of Vaccination.* By Dr. CARL BOTH. Boston, 1872.

4. *How to Prevent Smallpox: being Plain Facts on Vaccination, and Hints on Sanitary Precautions.* By MORDEY DOUGLAS, M.R.C.S. London, 1871.

5. *Notes on Smallpox and its Treatment.* By W. GAYTON. London, 1873.

6. *Report on the Smallpox Epidemic, 1871-73, as Observed in Cork Street Fever Hospital.* By THOMAS WRIGLEY GRIMSHAW, M.D. Dublin, 1873.

7. *La Variole au point de vue épidémiologique et prophylactique.* Par LÉON COLIN. Paris, 1873.



be my endeavour to show are the false views of the learned and scientific have also been injurious and hostile to the best interests of humanity. I allude to the promulgation of the doctrine that *contagion* is the sole and only cause of the extension or propagation of these maladies. From this doctrine have arisen those sanitary regulations which are productive of so much injury and such irreparable losses, both to individuals and to communities at large.”<sup>1</sup>

One of the terrible evils ascribed to the ordinarily received doctrine of the contagiousness of epidemic diseases is stated in the following forcible words of Sir F. Maitland’s despatch to Lord Bathurst on the plague at Malta in 1819:—“This system [of quarantine] cuts up by the root all those feelings of domestic life which are peculiarly endeared to the mind of man in a moment of sickness and distress, rends asunder all the usual bonds of society, and places the unfortunate patient in a situation of the most desolate isolation at the moment when the only remaining comfort of life exists in the kindness of natural friends and connections.” “Whether diseases be contagious or not,” adds Dr. Parkin, “it is better to stand shoulder to shoulder as soldiers do, and meet death like men, not like cowards and poltroons.” But, we may ask, are soldiers expected to stand “shoulder to shoulder” and allow themselves to be cut down by contagion as with a scythe, or mown down by an epidemic as with mitrailleuses, when by isolation they may escape the enemy and may have the opportunity of renewing the attack on another occasion?

Without further prelude we will discuss the subject of zymotics, defining the term, depicting some of the features of a well-known group of maladies, illustrating our remarks by that characteristic zymotic variola, and stating as we go along the ideas which reflection suggests.

By a contagious disorder, a zymotic disease, we mean a disease which “is in any way *communicable* from one person to another.”<sup>2</sup> Now, this class of diseases present certain characteristics which it is necessary to bear in mind, viz. the manifest introduction into the system of a virus, its lying dormant for a certain period, its developing into a form identical with that in the person first affected, and the enormous increase and multiplication of the poison. These distinctive features are not, however, always to be readily perceived, just as the offspring does not always present a striking resemblance to its parents. In both are to be found degrees of likes and dislikes, a correspondence in certain traits, a difference in others. So also it often puzzles

<sup>1</sup> Parkin, ‘Epidemiology,’ part i, p. 9.

<sup>2</sup> Watson, Sir Thomas, ‘Lectures on the Principles and Practice of Physic,’ 1871, vol. ii, p. 843.

the practitioner to observe one set of cosmical phenomena or one class of material conditions giving rise to apparently dissimilar zymotic diseases, as if one virus could produce two separate affections—when, for example, measles and scarlet fever attack houses on the opposite sides of a street, or when the former affects some member of a family and the latter others. Again, the immediate sequence of one fever on another in an individual, or the appearance of one and its giving place, so to speak, to another, without completing its ordinary course, and perhaps reappearing in the same individual after a temporary suspension of its existence—such observations as these are frequently made in practice, and are laid on the heap of unexplained phenomena, generally without an effort to analyse them. But it must also be borne in mind that each member of the human species is born apparently with a susceptibility to contract one or more of the diseases termed zymotic. “There is in the organism, most likely in the blood, some in-born principle or ingredient, clearly not essential to life and well-being, by which we are rendered liable to undergo these diseases.”<sup>1</sup> Furthermore, although suitable soil for the fructification of the seeds of zymotic diseases is always present everywhere, though such germs are also always to be found smouldering within very circumscribed limits, and notwithstanding the presence of media potent for the spread of these diseases, yet epidemics are not always present among us, nor are their character and type always equally virulent. In fact, the vegetable and animal worlds closely approximate in this direction also. Not only is it requisite for the spread of zymotic diseases that the seed and suitable soil be present, but the seed must be active (living), the soil must be genial, there must be sunshine and moisture, in short all the animal and cosmical conditions requisite for the reception and fructification of a germ. In the case of zymotic diseases, as in the vegetable world, extremes of temperature or excessive alterations in surrounding circumstances are inimical to the development of the germs. Granting these data, we shall now look for confirmatory evidence in the observations of those who have recently written on the subject.

By way of illustration the variolæ offer the most typical features. This group of fevers includes by far the best examples of zymosis, inasmuch as they are both contagious and inoculable, are communicable by direct contact and by indirect media, are recognisable in the lower animals and in the human subject, and they alone, of all contagious diseases, have yielded

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<sup>1</sup> Dr. Russell Reynolds, ‘A System of Medicine,’ vol. i, p. 240.



up their germ to microscopical research. While, then, in this group of zymotics the best possible opportunity is afforded for studying the life-history of a contagium affecting both human beings and the lower animals, the extent of our present ignorance of the essential properties, manner of growth, and mode of propagation of this one germ proves that it is impossible to study too diligently those most fatal and most general forms of disease termed zymotic. The latest literature on variola, which we have lying before us, treats of the subject microscopically and statistically. The microscopical aspect has particularly engrossed the attention of Dr. Bakewell, who describes the symptoms and pathological appearances of 250 cases "from notes taken at the bedside of the patients, or jotted down with the microscope on the table." Having dried and made a section of the skin of a Hindoo who died on the second day of the eruption Dr. Bakewell found (as is already known) the rete mucosum "forming the outer wall of the vesicles," each of which consisted of a membranous sac containing fluid. This fluid on microscopical examination was observed to contain epidermic cells, and floating about a few rounded corpuscles (a little larger than red blood-corpuscles), including one to four bright, well-defined nuclei, or two larger nuclei with nucleoli, also highly refractive globules of the same size as the nuclei, and others still smaller. These corpuscles tend to form groups of six or eight, and in persons belonging to the coloured races pigment-cells were seen. Dr. Bakewell, in twenty-seven cases, examined daily the contents of the vesicles with the microscope, and found "that the variolous corpuscles increase in number daily, and that a new form, not to be found on the first or second day, appears. This is a proliferous cell, large enough to contain three, four, or even five of the smaller corpuscles. These proliferous cells apparently burst and discharge their contents, as there are always to be found with them flat circular bodies of the same size, of an absolutely structureless membrane, and without any contents. I have also seen," he says, "in many cases one of the proliferous cells ruptured at one part, with one or two corpuscles just escaped from its cavity lying by its side, and others remaining within it."<sup>1</sup> The opacity of the fluid, observed on the seventh and following days of the disease, is owing to the "multitude of these solid bodies," which, with the proliferous cells, are most numerous at this stage of smallpox. "On and after the ninth day," remarks Bakewell, "the proportion of free nuclei or bright granules (or nucleoli?) and of proliferous cells diminishes gradually, the medium-sized corpuscles predominating; and on

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<sup>1</sup> 'Bakewell on Smallpox,' p. 4.

the tenth or eleventh day the corpuscles are all found to be much of the same size, having then arrived at what I believe to be their full growth." The tenth or eleventh is, in a normal variole, the latest day on which they can be observed without the addition of some reagent. In the dust even of smallpox wards the variolous corpuscles are detectable by the microscope. The proliferous cells were not discoverable in the severest hæmorrhagic cases. "This may arise," he says, "from the fact that in these malignant cases the patient never lives to that stage at which proliferous cells are formed."<sup>1</sup> Dr. Bakewell entirely discards the ordinary nomenclature—papule, vesicle, and pustule—and regards the "pock" in its entire existence as a vesicle. The same elements, he says, are observed at each stage of the disease, but at the first appearance of the eruption these elements "are few in proportion to the fluid in which they float, and at the close they are so numerous that the fluid is only sufficient to moisten them and keep them alive." But stronger evidence than what we have adduced (and this is all that Dr. Bakewell supplies) is desirable ere the opinion hitherto and at present held can be laid aside. On the contrary, the description just quoted appears to confirm the views most generally entertained that the leucocytes, white-blood-corpuscles, or variolous corpuscles (of Dr. Bakewell), in the later or pustular stage of the disease, give place to or undergo a pustular transformation.<sup>2</sup>

Regarding the microscopical appearance of the blood in variolous patients, Dr. Bakewell states that in three cases

"Corpuscles, exactly resembling those contained in the varioles, were to be found abundantly in the blood of a patient on the first day of the eruption, when it was very slight, and that none were to be found on the third day, when the eruption had become extremely copious and confluent" (p. 6).

In the classification of the varieties of the *variola humana* we meet with a considerable diversity of opinion; but it would not be right to overlook the suggestions, based as they are on clinical observation, made by Dr. Bakewell, who includes all cases of smallpox in the three following classes—First, those in which the contents of the varioles are such as have been described, and to these he applies the term of *variola simplex*, subdividing the class into "*discreta, semi-confluens, confluens, corymbosa, and per-confluens.*"<sup>3</sup> Secondly, when in addition to the elements

<sup>1</sup> 'Bakewell on Smallpox,' p. 39.

<sup>2</sup> The microscopical characters of the contents of variolous vesicles have been also fully described by Glüge (in 1838), Keber, Klebs, E. Wagner, Weigert, Cohn, and others, and a short account of their observations is to be found in the 'London Medical Record,' 1873, p. 55.

<sup>3</sup> Those interested in this subject will find an explicit description of the per-



of *variola simplex*, red blood-corpuscles are found in the varioles, the disease is *variola hæmorrhagica*, including the petechial form.

"This," he says, "presents just the same varieties as to extent or form of eruption as *variola simplex*. Thirdly, in which the varioles are very watery, the number of corpuscles found in them is very small, and they do not become yellow and form scabs, but dry up in wrinkles or separate (when confluent or corymbose) large patches of epidermis from the cutis, much as a blister might do. These detached portions of epidermis fall off if the patient survive, leaving the skin in the coloured races quite white, from the destruction of the rete mucosum and its layer of pigment-cells. This form may be called *V. aquosa*. It has, like the others, its varieties as to amount of eruption."<sup>1</sup>

The first and second of these groups are sufficiently well described in the ordinary text-books on medicine, but *variola aquosa* is, indeed, such a very rare form of the distemper as to induce us to quote Dr. Bakewell's description. In his experience, also, this variety is seldom met with. *V. aquosa* is characterised, he says—

"By large watery vesicles, many of which will run together in patches, thus becoming corymbs. The contents are somewhat different from those of *V. simplex*. There are more free nuclei and bright nucleoli, and I never found any proliferous cells. The proportion of corpuscles to fluid is, on the seventh or eighth day, not one sixth that of the ordinary variole. The varioles do not burst, they shrivel and dry up."<sup>2</sup>

This classification may be regarded in some respects as more comprehensive, or, perhaps, even more expressive of meaning, than the subdivision of smallpox ordinarily described in books. Here, however, the old bugbear reappears. In overcoming an evil we do not necessarily do good. We amend one trouble, and create another in no way inferior. By the multiplication

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confluent variety of *V. simplex* at p. 20 of Dr. Bakewell's pamphlet. This name he applies to a rare form of smallpox, in which "The *primary* fever is of a very intense kind, and the pain in the back and limbs are extremely severe. The first appearance of the eruption is hardly perceptible in blacks, but in white or light-coloured people it is exactly like a scarlatina rash—intensely red, and disappearing for a moment on pressure; equally diffused over the whole body. The next day it presents a glistening or shiny appearance, from the effusion of a clear fluid under the epidermis; the fever continues unabated. From the first day of the eruption the symptoms of blood-poisoning are very marked; on the second or third day a state of semi-coma sets in; from the third to the fifth day, in the worst cases, death occurs in a state of complete coma, with stertorous breathing." A similar variety of smallpox appears to have been observed by Dr. W. Zuelzer, of Berlin, who has described it in the 'Berliner Klinische Wochenschrift,' No. 51, 1872.

<sup>1</sup> 'Bakewell on Smallpox,' pp. 9, 10.

<sup>2</sup> Ibid., p. 26.

of names, or by the formation of varieties in a group of diseases, individual and minute differences obtain a more prominent place than they deserve. The number of spots forming the eruption weighs heavier in nomenclature than the source of the poison which induces the disease.

Before leaving this author we cannot refrain from adding our quatum of emphasis to his remark—

“That until the late epidemic taught us a lesson we were in the habit of trusting exclusively to vaccination, and neglecting those other prophylactic measures which are just as needful in this as in other infectious fevers. We have looked too much to the scars on the arm, and thought too little of the previous habits, mode of life, and immediate surroundings of the patient.”

In all sanitary matters, however, the same English instinct crops up. Those who from natural habit or as the result of patient study have the faculty of looking round and round an object, of sifting out and hunting after the various causes of a disease, of not resting satisfied with the discoveries revealed by one light only, such far-seeing men have, indeed, a hard struggle to carry on when investigating the source of an epidemic. English sanitary authorities have too frequently been satisfied with tracing any epidemic (whatever may be its pathological characters) to a neighbouring dust-heap, or to the effluvia of decomposing vegetable and animal matter from a midden within a mile of the seat of the disease; whereas the real *origo mali* is discovered by an independent searcher in the sewer passing beneath the house or in the milk supplied to the household. Too many of our sanitary officials spend time, thought, and money, over the conversion of privies into water-closets, or of waterclosets into dry-earth closets, and pay no attention to the overcrowding of houses or to the isolation of the diseased. Our legislature insists on fine or imprisonment being inflicted on the seller of adulterated milk (even though the adulteration be simply water), but no punishment is meted out to the careless parent who endangers many lives by allowing his child, scarcely convalescent from an exanthema, to return to school and disseminate a disease which will disable many and kill some. Severe chastisement follows the admixture of iron filings with tea, and such a commodity it is not allowable to dispose of to the public; but the very vessel which carries such a cargo may also import sufferers from zymotic disease, and these can be distributed freely among the teeming multitudes of a seaport town, and no sanitary authority objects. But the dilemma does not end here.

While contagiousness is admitted, while certain cosmical conditions, certain social arrangements, and like well-known



circumstances, are held to foster and propagate zymotic diseases, and although everyday experience proves that certain members of this class find a genial soil for development in this country, while others feel the English climate inimical to their growth, yet our quarantine arrangements exclude the latter, but do not prevent the importation of the former. Scarlet fever and measles slay their thousands yearly in England, no absolute means of protection against them is known, and yet we allow patients suffering from such highly contagious diseases to be brought into our ports and to be admitted into the lodging-houses and hotels of our seaports (which are in general very crowded), while the yellow flag is hoisted over the ship which may have come last from a port in which yellow fever rages, and the vessel is at once put into quarantine on which a death from cholera has occurred.

No form of epidemic disease illustrates this point more strikingly, or cries more urgently for reform in the present manner of investigating the sources of zymotic infection, than does smallpox. The recent outbreak of smallpox in England (and we find the same observation made by Dr. Bakewell) was characterised by a greater prevalence of the hæmorrhagic variety of variola than was known formerly, and by a larger number of instances of smallpox, and of deaths from smallpox among the vaccinated than is explainable by former experience. So also Dr. Grimshaw remarks that during the epidemic of 1871-72-73, in Dublin, the "cases of smallpox must have numbered 12,000, probably 15,000, and the disease caused 1647 deaths in the Dublin registration district." And why is this so? Because too much attention has been paid to the number, size, and form of vaccinal cicatrices, and none to the character and age of vaccine lymph. More stress is laid on the necessity of possessing four scars than on the purity of the vaccine lymph: how long since the lymph was derived from the cow, through what variety of constitutions it has been transmitted and how far its protective efficacy has been affected by these constitutional differences, are points which receive comparatively slight attention. If a tendency to disease is transmitted from parent to offspring—and such it is held to be by all pathologists—and this "hereditary predisposition" can be communicated only through the medium of the blood, is it not logically and physiologically correct to conclude that the serum of such blood is proportionately affected from the same cause, and that the vaccine lymph produced from this serum is likewise different from the original lymph yielded by the cow? The evidence opposed to such reasoning is that in appearance, efficacy, and effect, the vesicles produced by the vaccine ordinarily in use, and which

has been transmitted through numberless human beings, are identical with the vesicles caused by the application of lymph from the cow. But with equally good reason two apples which exhibit similar physical characters may be said to have an identical chemical constitution ; but such a conclusion would be readily rejected. " I examined personally a considerable number of cases in one district of the island," says Dr. Bakewell, " and found no less than 31 out of 90 were those of children vaccinated since the commencement of the epidemic" (p. 28). Again, Dr. Russell (' Glasgow Med. Journ.,' Nov., 1872) records that of 972 cases of smallpox 623 had been in one way or another vaccinated. " As to the nature of the vaccination marks," remarks Dr. Grimshaw, " whether good or bad, I have not been able to find that there was any relation between the nature of the mark and the severity of the disease."<sup>1</sup>

These remarks are intended, not to depreciate the value of vaccination, but to draw attention to points in relation to this great discovery which are apt to be overlooked. Vaccination is a valuable and almost certain protection against smallpox, but it should not be held to be a corrective also of that unhealthy state of the system which generally accompanies and often predisposes to variola. " It may be merely a coincidence of percentage mortality," says Grimshaw, " but it is a remarkable fact, that in the cases under consideration vaccination reduced the mortality of confluent cases to that of discrete unvaccinated, and that of malignant to that of confluent unvaccinated cases." The differences, to which we draw attention, between vaccine transmitted through hundreds of human beings and that derived directly from the cow, these distinctions are, no doubt, so slight as to defy detection by the ordinary modes of research, but they may, notwithstanding, be realities. Vaccine lymph derived from separate sources may be identical in every respect except in the faculty of affording protection, and this is demonstrable only by observing carefully the effects of epidemics. All vaccine lymph is protective against smallpox, but all may not afford an equally good or lasting protection. If vaccine is to yield the protection it is able of affording, its composition should be as nearly perfect as it was when derived from the cow, and on an outbreak of smallpox, as on that of other zymotic diseases, special attention should be paid to general sanitary arrangements.

We proceed next to notice Dr. Both's little treatise, containing no signs of reasoning or observation, not even amusement ; it is truly hard to understand how such a book has appeared.

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<sup>1</sup> Grimshaw's ' Report,' p. ix.



Wisely, indeed, in consideration of his readers' time and patience, has the author summed up his conclusions in a few sentences, one or two of which will reveal the worth of this book, *e. g.* :

"That I have discovered the following facts, and maintain that smallpox consists in the escape of *superfluous* albuminous substances into the tissues of the periphery of the nervous centres of the body, caused, in the first place, by the want of salt. That the *proper* use of salt is the scientific and most certain preventive of smallpox, both in the theory and practice, that I have any knowledge of."

And so on, going more and more deeply into a mire of absurdities.

Mr. Gayton's pamphlet is intended to instruct "more particularly junior members of the profession and those who have not been brought much in contact with smallpox." It would have been well, therefore, if this author had kept to the long-established landmarks in his classification. His definition of "malignant" or "black smallpox" appears to us incorrect; and this variety, as also the kind he terms "*variola benigna*" are included in the ordinarily accepted "*hæmorrhagic*" and "*discrete*" forms of *variola*. The charts showing the variations of the pulse and temperature in the different varieties of smallpox exhibit a remarkable consonance or relation between the two, each increase of ten beats in the pulse corresponding with an elevation of one degree in temperature. The tables, which classify the mortality from smallpox in accordance with the number of vaccinal cicatrices presented by patients and with their age, confirm the already well-established conclusions that four vaccinal cicatrices guarantee twice as great a security as two, and that the extremes of age are the most fatal periods.

To the treatment of smallpox Mr. Gayton devotes several pages, but no new information is thereby afforded, except that he found large doses of quinine to reduce the temperature very decidedly; a fact indeed already observed by others. "Many in whom recovery seemed hopeless succeeded in establishing convalescence; but it is unwise to extol a remedy" (quinine) "which has not yet stood the test of long experience."<sup>1</sup> On the important question as to the date at which the prophylactic influence of vaccination is obtainable, this author's observations deserve attention :

"Over and over again," remarks Mr. Gayton, "we have seen children and adults who have been vaccinated as a precautionary measure (smallpox, perhaps, having broken out in the house in which they were living), in whom three, four, and five good recent

<sup>1</sup> Gayton, W., 'Notes on Smallpox,' p. 21.

vaccinia vesicles were visible ; nevertheless the disease itself passed through the usual stages and was unmodified, and several unfortunately fell victims to the attack."

After giving the method a trial, and obtaining in every instance a most unsatisfactory result, "We consider it," he says, "a very happy means to bring vaccination into the utmost discredit."

The sequelæ of smallpox are very fully detailed in this pamphlet, and the treatment recommended differs in no important particular from that inculcated in our text-books.

The experience of the Homerton Smallpox Hospital further confirms the generally accepted opinion that epidemic diseases rarely attack those suffering from constitutional affections. The remarkable fact has been observed, over and over again, that the subjects of phthisis and cancer escape zymotic influence, while the seemingly robust succumb. A paralytic will escape, although surrounded by sufferers from smallpox, while the healthy become ready victims. But the problem becomes more difficult of solution in the case of the healthy, who, coming into the focus of an epidemic, are attacked by it, while those residing among such epidemic influences escape.

In discussing the contagiousness of smallpox—What is contagium? How does it spread? When is it most diffusible? and so on, Mr. Gayton furnishes some interesting and striking examples derived from his own experience as well as from the writings of more distinguished observers.

The consideration of this matter brings us to the point from which we started viz.—What is contagium, what is its essence, and what its life-history? It is not of much importance which feature of the zymotic influence is first portrayed; and as Dr. Léon Colin's brochure affords a valuable contribution to our present knowledge of smallpox, we prefer to notice in succession the several points to which he draws attention. Dr. Colin, in the Bicêtre of Paris, was supplied with opportunities such as are very rarely indeed met with. This we learn from his statement, that between October 12th, 1870, and April 1st, 1871, he had under his charge 7578 cases of smallpox. But another interesting circumstance to be noticed is that a more remarkable outbreak of smallpox than the epidemics of 1822, 1834, and 1846, should precede one of those national convulsions from which France has more than once suffered. Accordingly, the smouldering embers of a pestilential fire, which under ordinary circumstances would have been slowly wafted from place to place, were hurriedly accumulated within the narrow limits of a besieged city. We have, then, to deal with an epidemic which was stamped out among a sparse population by the very



means which introduced it, and intensified its influence among the teeming inhabitants of a metropolis.

The first characteristics of smallpox, on which Dr. Colin remarks, are its transmissibility and the effect of the congregating of human beings on the diffusion of the poison.

"The cessation of smallpox," he says, "may take place in spite of the presence in the infected district of a great number of individuals who offer no habitual conditions of preservation against it. An instance of this was seen in the recent epidemic, when, at the conclusion of the war, the army of Versailles, with a large number of citizens and strangers, returned to Paris without occasioning any new outburst of the epidemic, which was then dying out, and many germs of which still existed."

On this point Dr. Colin considers himself supported by the experiences of Dr. Gavin Milroy, in Australia; but he proceeds to remark that the transmissibility of smallpox, at all times and in every season, is further proved by the inoculability of the disease, equally well during epidemic periods and during their intervals.

Now, this very trait of variola distinguishes this zymotic from other zymotics, and it upbraids with conceit the quasi-scientific observer who does not draw a distinction between the transmission of contagium by inoculation and its diffusion by ethereal media. It is probable that at all times a very considerable proportion of the human race are susceptible to the reception of smallpox by *inoculation*, and this quota of the population is probably neither increased nor diminished in amount during variolous epidemic periods. But an equal percentage of human beings are not susceptible to the variolous influence as communicated by what is ordinarily termed *contagium*, else smallpox epidemics would be ever raging. This enigma is no more explicable by our present knowledge than is that other characteristic of the smallpox virus which causes a mild form of the disease to be induced by inoculation, even though the lymph be derived from a severe type of the affection. "The oscillations in transmissibility," remarks Dr. Colin, "appear to be attributable to the effect of meteorological influences on the volatile contagium and on the specific miasm engendered by variolous patients, which is the principal, if not the sole cause of the disease."

The propagation of smallpox (this holds good of all zymotics, and no one can deny it) depends chiefly upon the facility of intercourse among nations. This Dr. Colin illustrates by his observation that the epidemic he described appeared in Paris during October, 1869, spread over France during 1870, and broke out in Germany, England, and the United States at the

commencement of 1871, raging, as he says, simultaneously in Philadelphia, London, Hamburg, Berlin, Vienna, and Rome. Granting, then, that the most common and most powerful agent in the diffusion of smallpox is free intercourse among nations—those boons of modern civilisation, the railroad and the steam-boat—can we by this means account for the distribution of an epidemic, like that just described, over a very large portion of the globe, and that in a comparatively short space of time? Or, are we compelled to hide our ignorance behind such a hazy mask as is implied by the expression “meteorological influences”? If “free trade” and “commercial enterprise” are not sufficiently potent factors in the diffusion of smallpox, do we reach any nearer the truth by trusting to “trade-winds” and the ruthless son of Astræus and Aurora? Now this ancient and natural explanation of the origin of zymotics through cosmical phenomena, and their spread by aerial media, has been recently revived by more than one scientific inquirer, not the least weighty of whom is Dr. Balfour Stewart, who (in ‘Nature,’ September 26th, 1872, p. 432) remarks, “The researches of Baxendell, Meldrum, Smyth, and others, go to show that the convection currents of the earth are influenced by the state of the solar surface. Now, surely anything that influences the motions of our atmosphere may readily be supposed to influence the distribution and activity of those disease germs that are now believed to be present in the atmosphere.” Dr. Stewart gives us the dates, 1517, 1622, 1708, as “years of maximum auroral disturbances;” and “if they correspond,” he remarks, “approximately with years of maximum sun spots, we should expect the distances between them to be divisible by 11·2, which Wolf, as well as De la Rue, Stewart, and Loewy agree in representing as the solar periods.” Now, on examining an elaborate table of smallpox epidemics which lies before us, we find that the oldest smallpox epidemics, concerning which we have reliable information, occurred in Iceland; and it is recorded in the annals of that island, that during the following years variola raged very virulently: viz., in 1257 to 1258, 1291, 1347 to 1348, 1430 to 1432, 1511, 1616, and 1707; which last is described by Stephenson as *the great epidemic*. On computing these dates by solar periods we observe a wonderful conformity, *e. g.* :

From 1258 to 1291 is 33 years, or 3 solar periods.

|   |      |   |      |   |     |   |   |     |   |
|---|------|---|------|---|-----|---|---|-----|---|
| „ | 1291 | „ | 1347 | „ | 56  | „ | „ | 5   | „ |
| „ | 1348 | „ | 1430 | „ | 82  | „ | „ | 7·2 | „ |
| „ | 1432 | „ | 1511 | „ | 79  | „ | „ | 7   | „ |
| „ | 1511 | „ | 1616 | „ | 105 | „ | „ | 9   | „ |
| „ | 1616 | „ | 1707 | „ | 91  | „ | „ | 8·1 | „ |



In examining the dates at which smallpox epidemics have been recorded in other countries, such a definite and prolonged interval as the above cannot be discovered. This is readily explainable by the free intercourse between different nations, and their connection together in one main-land.

The next feature of smallpox to which Dr. Colin draws attention is the influence of aggregation on the virulence of the poison. No better opportunity could have been afforded to prove (if proof were necessary) that the isolation of those attacked by smallpox prevented the spread of the scourge; and on this point Dr. Colin's opinion coincides with that of all careful observers. But "it is an error," he says, "to consider smallpox, which is above all a contagious disease, as belonging to the group of infectious diseases which can and will certainly be aggravated by the sole fact of the excessive number of patients."<sup>1</sup> His observation was that the accumulation into a limited space of large numbers of sufferers from smallpox did not increase the virulence of the disorder, and, accordingly, that such patients did not exhibit a more severe type of the disease than those treated in their houses or in smaller hospitals. The mortality in the Bicêtre, where sometimes as many as 1300 patients were under treatment at one time, was not greater than that of smaller hospitals or of ambulances devoted to variolous cases. This, he remarks, is a new proof "that in purely virulent affections having a specific cause, with an evolution determined, so to speak, beforehand, the fate of the patient depends generally on the energy with which he is first impressed by the contagious germ, rather than on the circumstances in the midst of which he undergoes the disease."<sup>2</sup> Those physicians who have had the largest experience in the treatment of fevers will probably agree in the general import of this last quotation. In all hospitals the aggregation of patients stricken with one type of fever indicates that the opinion ordinarily held is that an accumulation of one fever poison in a ward does not aggravate the disease in those suffering from that disease. To occupy a ward with scarlet fever cases alone does not intensify the type of this fever in the case of the inmates of such a ward; but to introduce a patient with measles into a ward filled with scarlet fever, would be exposing him to the influence of another poison, and would intensify the action of the measles poison. It is most important that this distinction be recognised, and that it guide our practice.

Again, certain zymotics are no doubt rendered more destruc-

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<sup>1</sup> Colin, Léon, 'La Variole,' &c., p. 38.

<sup>2</sup> Ibid., p. 38.

tive, and are perhaps even originated by the crowding together of human beings; and in these instances it is found very prejudicial to confine within limited space patients suffering from such diseases. To place in a ward a number of typhus cases aggravates seriously the type of the fever. Again, to accumulate together cholera patients is deleterious. Now, how is it that neither profuse inoculation with the variolous virus, nor excessive exposure to smallpox contagium, induces worse effects than a limited dose of this poison seems to generate? Yet in the case of many organic and inorganic animal and vegetable poisons, the amount bears a definite proportion to the effects produced. In short, the more we reflect on the mysterious life-history of contagium, the more puzzled we become; the more we reach after this subtle principle the farther it seems from our grasp; the more carefully we lay hold of one fact after another, the greater number of new facts spring up mushroom-like before us; and yet as practitioners we have daily to contend with the enemy, and require to compel him to unmask his many-featured existence. There is no class of diseases so fascinating to theorists as zymotics; nor any which elicits so much industrious and laborious research from the ardent physician; nor any which holds forth fairer promises to the delighted experimenter, and then heartlessly strikes down his fondest hopes; nor has any form of disorders been more thoroughly investigated, nor more indefatigably sifted, than that of which we have been treating. But, from our remarks, it will be evident that he only

“Who closely pores o’er Nature’s page,  
Science extends from age to age.”

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### III.—Food and Dietetics.<sup>1</sup>

Food and dietetics have of late engaged much of the attention, not only of professional and scientific men, but also of the public at large. The periodical press also has kept alive no little interest in the subject, by reporting numberless cases of

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<sup>1</sup> 1. *Traité de l’Alimentation dans ses Rapports avec la Physiologie, la Pathologie, et la Thérapeutique.* Par le Docteur JULES CYR. Paris, 1869.

2. *Foods.* By EDWARD SMITH, M.D., LL.B., F.R.S. London, 1873.

3. *A Treatise on Food and Dietetics, Physiologically and Therapeutically considered.* By F. W. PAVY, M.D., F.R.S. London, 1874.

4. 35th & 36th Vict., ch. 74 (commonly known as *The Adulteration Act*, 872).



the prosecution of vendors of alleged impure articles of food and drink ; and when it is remembered that for one prosecution for the sale of an adulterated article, a dozen or so of such articles have been analysed under the new Adulteration Act, it will be evident that traders in food products must, during the last two years, have had an uneasy time. New researches into the influence of foods and drinks upon the human frame have attracted much attention from the profession ; and a need has been felt for a popular, yet scientific, work on food and dietetics. The increasing cost of procuring the more necessary articles of ordinary dietary has also enforced the necessity of looking to distant countries and colonies for our supplies of beef and mutton, and thus the skill of inventors has been engaged in devising new methods of preserving food.

It may perhaps appear almost superfluous to urge upon medical men a more careful and accurate study of food and dietetics, yet it may not be amiss to quote a few passages bearing on this point from the books we have placed at the head of this article. Dr. Cyr says :

“La question de l'alimentation envisagée au point de vue de la physiologie, de la pathologie, et de la thérapeutique, doit à juste titre préoccuper le médecin, et me paraît mériter plus d'importance qu'on ne lui en accorde. La plupart des praticiens la traitent en suivant les données de l'empirisme et de la tradition vulgaire ; chacun croit pouvoir en raisonner avec compétence ; aussi la quantité de préjugés qui circulent sur cette matière est-elle considérable” (p. vii).

He speaks of France alone, but his words are not altogether inapplicable to our own country.

Dr. Pavy writes :

“From the fact that the subject of Food is one of deep concern, both to the healthy and the sick ; that the information which has been obtained during the last few years has completely revolutionised some of the cardinal scientific notions formerly entertained . . . . I have been encouraged to think that the task I have undertaken may not be deemed superfluous” (p. v).

Twenty or thirty years ago Liebig's well-known views respecting the functions of various kinds of food, and his division of them into respiratory, nitrogenous, and mineral, obtained all but universal acceptance among medical men. It is true that a few thoughtful men did not perceive the force of Liebig's argument, urged in his clear, incisive, and somewhat intolerant manner ; but they were in a decided minority. Facts at large accorded, or seemed to accord, with the illustrious German's theory, and with a little pressure facts apparently opposed to

his views were reduced to conformity with his system. The clear enunciation of the doctrine of the conservation of energy, and the more exact study of the quantitative relations of food and the excretions, made, however, sad havoc with Liebig's theory, and it was soon perceived that either Liebig's distinction between the functions of the respiratory, nitrogenous, and alimentary principles must be given up, or that it must be accepted in a very modified form. The now celebrated observations of Drs. Fick and Wislicenus gave a decisive blow to Liebig's theory that muscular work is dependent on, *and proportioned* to, the destruction of muscular tissue by oxidation. Dr. Pavy's book contains a very full and lucid account of these experiments of the illustrious Swiss professors, and will amply repay perusal. He says—

“Liebig's idea, then, upon this point is very precise. He considers that nitrogenous matter may contribute towards heat-production, but that it must first pass into the condition of tissue before it can do so, and that it is in the wear and tear of tissue that occurs the splitting up of the compound, so as to lead to the production of urea for excretion on the one hand, and the liberation of carbon and hydrogen for oxidation on the other.

“The facts which have already been adduced suffice to refute this doctrine. Indeed, it may be considered as abundantly proved that food does not require to become organised tissue before it can be rendered available for force-production” (p. 59).

He admits, nevertheless, as all physiologists admit, that wherever vital operations are going on, there nitrogenous matter is present, forming the spring of vital action; and that the primary object of nitrogenous alimentary matter may be said to be the development and renovation of the living tissues (p. 37).

It is in the relation of nitrogenous matter to force-production, and not to the part which such matter plays in the development and renovation of the tissues and the maintenance of the secretions, that the views of physiologists have undergone a revulsion. Perhaps the dogmatism with which Liebig promulgated his own views led to the reactionary wave, which culminated in Traube's directly contradictory axiom respecting the source of muscular and nervous power. According to him, a muscle is not destroyed or consumed by its contractile action; but the force evolved by muscular contraction is affirmed to be due to the oxidation of non-nitrogenous organic matters, the muscle merely serving as the instrument for the conversion of the generating oxidation-force into the resultant motor force, just as the machinery of a steam-engine is the medium by which the oxidation-force (heat) evolved by the combustion of



coal is converted into the motor force of the engine. Dr. Pavy very clearly puts the argument representing the question to be solved (p. 38), and gives very copious details of the new classical researches of Fick and Wislicenus on the quantity of nitrogen excreted before, during, and after work, and the valuable supplementary researches of Dr. Parkes and others. In this respect his work presents a favorable contrast with the other volumes we have enumerated at the head of this article. Dr. Cyr dismisses the subject very curtly, dividing foods into two great classes—the nitrogenous or plastic, and the non-azotised or respiratory—"parce qu'ils sont plus particulièrement utilisés pour la production de la chaleur." Liebig is blindly followed, and the views of Fick and Wislicenus, and of Parkes, are not even alluded to. Dr. Edward Smith's volume on 'Food' is intended to be succeeded by another on 'Dietetics,' so that he perhaps reserves what he has to say on the subject of the functions of food. He says, however, that

"It may thus be shown that the division of foods into the two great classes of flesh-formers and heat-generators is not to be taken too incisively, for whilst a food is renewing flesh it also produces heat; and whilst the heat-generating food is acting, it may also produce a part of flesh in the form of fat; but although they are so closely associated in their vital work, the leading characteristic of each kind is so marked as to warrant the classification which Liebig has formulated" (p. 6).

So that we may take it that he is in some sense a follower of Liebig. But his own researches previously recorded have shown that even severe labour did not increase the excretion of urea to a material extent; and have themselves tended to refute Liebig's views.

The key to the full appreciation of the nutritive value of food lies, we believe, in the full acceptance of the view so ably laid down by Fick and Wislicenus, enunciated in this country first of all by Dr. Frankland, and carefully elaborated by Dr. Parkes. It is thus concisely stated by Dr. Pavy:—

"While nitrogenous matter may be regarded as forming the essential basis of structures possessing active or living properties, the non-nitrogenous principles may be looked upon as supplying the source of power. The one may be spoken of as holding the position of the instrument of action, while the other supplies the motive power. Nitrogenous alimentary matter may, it is true, by oxidation contribute to the generation of the moving force, but, as has been explained, in fulfilling this office there is evidence before us to show that it is split up into two distinct portions, one containing the nitrogen which is eliminated as useless, and a residuary non-nitrogenous portion which is retained and utilised in force-produc-

tion. It is true, also, that non-nitrogenous matter may be applied to tissue-formation, but it is probable that, in doing so, it is simply for the purpose of being stored up for subsequent appropriation to force-production, according as circumstances may require" (p. 81).

It is not, then, in the amount of metamorphosis of muscular and nervous tissues undergone and measured by the quantity of urea excreted, that we must look for the measure of force-production, but rather in the quantity of carbonaceous food oxidised. The difference between the older views of the physiologists of Liebig's school and the more modern views lies in this, that, whereas the former referred the chief if not the only source of heat to the oxidation of carbon and hydrogen, and mainly to the oxidation of the carbon and hydrogen of non-nitrogenous food, the principle of the conservation of energy induces us now-a-days to ascribe all force-production, and hence of necessity mechanical power, *par excellence*, to the same source. We need only refer to the classical experiment of Grove, by which the force of the sun's rays was converted into chemical effects upon the silvered plate, into electricity and into heat in the wire, into magnetism in the coil, and also into motion, as a proof of the correlative and mutual convertibility of the physical forces. Joule and others have given quantitative estimates of the equivalents or values in exchange of those forces; so that the hypothesis in question now rests upon a sure and determinate basis.

We have already seen how it has been satisfactorily demonstrated that there is no relation between work done by the body and urea excreted. But even when a direct relation was supposed to exist between these, it was generally believed that the elimination of carbonic acid was increased by muscular exertion. In 1859 Dr. Edward Smith put this supposition to rigid experimental proof, and in the work before us (pp. 12, 13) tables are given showing in a striking manner the relations between work done and carbonic acid eliminated. Pettenkofer and Voit subsequently extended Dr. Edward Smith's researches, and with improved apparatus prolonged their experiments over periods of many consecutive hours. Their experiments show that, taking the mean during periods of rest and periods of work, the difference is expressed not by an alteration in the excretion of urea, but in an increased discharge of carbonic acid during the period of work. Probably the oxidation of hydrogen and consequent production of water bears the same relation to muscular work as the oxidation of carbon, but this is perhaps not capable of being demonstrated experimentally. We are entitled, then, to consider it as something more than probable that to the oxidation of carbonaceous food may be



referred the production of force; and this view is adopted by Dr. Pavy in the volume under notice. He quotes from the celebrated article of Fick and Wislicenus the following happy simile:

“A bundle of muscle-fibres is a kind of machine, consisting of albuminous material, just as a steam-engine is made of steel, iron, brass, &c. Now, as in the steam-engine coal is burnt in order to produce force, so in the muscular machine fats or hydrates of carbon are burnt for the same purpose; and in the same manner as the constructive material of the steam-engine (iron, &c.) is worn away and oxidised, the constructive material of the muscle is worn away, and this wearing away is the source of the nitrogenous constituents of the urine. This theory explains why during muscular exertion the excretion of the nitrogenous constituents of the urine is little or not at all increased, while that of carbonic acid is enormously augmented; for in a steam-engine moderately fired and ready for use the oxidation of iron, &c., would go on tolerably equably, and would not be much increased by the more rapid firing necessary for working, but much more coal would be burnt when it was at work than when it was standing idle” (pp. 93, 94).

It must not be supposed, however, that nitrogenous food cannot contribute towards force-production. Doubtless it does so contribute to some extent. Dr. Pavy thinks that where nitrogenous food does not go to build up and renovate tissue, it nevertheless may undergo (probably by the action of the liver) a splitting up into urea for the one part, which carries off the nitrogen as an unavailable element, and into a slightly oxygenated hydro-carbonaceous residue for the other, which may, he thinks, be looked upon as applicable in the same way as primarily ingested non-nitrogenous matter to force-production. He is further of opinion that this force-production probably arises from the oxidation of hydro-carbonaceous matter existing in the muscular tissue, rather than from oxidation in the blood as it is circulating through the capillary vessels of the muscle, and in this we are inclined to agree with him.

The assimilation and absorption of carbon is a point which has received much attention from physiologists, and one of our authors, Dr. Pavy, has done useful work in elucidation of the matter. Although his views are controverted by Bernard, the English physiologist's experiments have not been, as we conceive, refuted in a satisfactory manner by the illustrious Frenchman, whose name attaches perhaps too great weight to the views he advances. As Dr. Pavy's opinions are perhaps less generally known than their importance deserves, we cannot do better than give a condensed account of what he has to say for them.

All the carbo-hydrates (sugar, starch, &c.) reach the circulation in the form of saccharine matter, and in this form pass on by the portal vessels to the liver. Now the sugar which reaches that organ must be either metamorphosed or destroyed by it, for if this were not so the saccharine matter would appear in the urine; sugar injected into the general circulation speedily appears in the urine. Nevertheless it is indubitable that in health no sugar, or only the minutest traces of it exist in the urine; although, if large quantities of sugar be taken fasting, a temporary diabetic condition may result. Liebig, Bernard, and others, hold, however, that the destruction of saccharine matter takes place in the lungs; but it is difficult to reconcile this with the fact, that sugar injected into the jugular vein soon makes its appearance in the urine. The liver, then, we may assume, is a great organ for arresting sugar; or, at all events, this is one of its functions. Dr. Pavy holds that the now well-known amyloid substance met with in the liver, and indeed forming a considerable proportion of the bulk of the hepatic cells, is formed by assimilation of the arrested sugar, which is indeed metamorphosed by the liver into the amyloid substance. In proof of this he adduces striking experimental evidence, showing that an increase in the amount of carbo-hydrates ingested is followed by an increase of amyloid substance in the liver. The well-established fact that amyloid substance is deposited in the liver after a purely animal diet, though in diminished amount, is accounted for by a splitting up of the nitrogenous principles into amyloid substance, and a complementary ureal residue which passes on into the general circulation, and thence finds its way into the urine. This hypothesis renders a satisfactory account of the appearance in the urine of an increased quantity of urea after the ingestion of large quantities of nitrogenous food. The liver, then, is not only the organ for converting sugar into amyloid substance, but is also the viscus which breaks up the nitrogenous molecules, and from them produces the amyloid substance also.

This view receives support from the remarkable observation made by Dr. Sydney Ringer, that in diabetes, when a purely animal diet is used, the ratio of sugar to urea in the urine is constant. As in diabetes there is a defective power to use the carbo-hydrates, which escape by the urine, if the theory be correct, we might expect that the ratio of sugar to urea, under a nitrogenous dietary, would be practically constant—the broken up nitrogenous molecules would be excreted unchanged.

That the carbo-hydrates once converted into amyloid substance undergo a further metamorphosis, and appear ultimately as fat, appears to be certain; but as to how this conversion is



effected physiologists are at present unable to say. On this point Dr. Pavy is vague and obscure, and thinks that the process requires the co-operation of nitrogenous and saline matter, and that it is probably through the medium of the change, excited by the metamorphosis of the former, that the result is brought about. He appears to regard the amyloid substance as a product intermediate between the carbo-hydrates and fat; and that not only does the liver form the amyloid substance, but, further, that it is the agent for its conversion into fat. He is also inclined to think that the carbo-hydrates do not contribute to force-production by undergoing direct oxidation in the system, although there is no direct experimental evidence of this view. We imagine that a considerable number of physiologists will agree with him.

The part which alcohol plays in the human economy is a subject of great interest, and receives considerable attention in the works before us. Dr. Edward Smith has himself laboured at the question of the mode of elimination of alcohol from the body. The time has long since passed when Liebig's dictum, "alcohol stands only second to fat as a respiratory material," could be received unquestioned, based as it was upon no physiological evidence, and supported by a supposed chemical analogy alone. We question, indeed, very much, whether the teetotallers did accept Liebig's views; and the mere fact that alcohol, though in its composition intermediate between the fats and carbo-hydrates, yet differs from these most markedly in its more obvious physiological effects, ought to have at once refuted the hypothesis that alcohol acts simply like other compounds of carbon, hydrogen, and oxygen. We cannot agree with Dr. Pavy when he says :

"It was one of Liebig's propositions that it (alcohol) is consumed by oxidation like any other non-nitrogenous alimentary principle. . . . That alcohol should occupy the position thus defined seemed so reasonable that Liebig's view originally met with general and unquestioned acceptance" (p. 121).

For this view appears to us to be far from reasonable. We may, however, be looking at the question too much in the light of modern experience to form an impartial judgment.

When brilliant but fallacious theories are accepted merely because they are advanced by some brilliant thinker, the reaction from them is sure to be most complete; so when Lallemand, Perrin, and Duroy gave forth to the world that alcohol is not a respiratory food at all, but is excreted from the body, after ingestion, in an unchanged state, their results met with speedy acceptance at the hands of physiologists. Dr. Cyr appears to

be an ardent believer in the conclusiveness of the researches of the French experimenters. He says:

“Trois points dominant dans le rôle que joue l’alcöol dans l’économie: — 1°, sa non-destruction, du moins dans les circonstances ordinaires, physiologiques; 2°, son élimination lente et en nature par les émonctoirs principaux (poumons, surface cutanée et reins); 3°, enfin sa localisation, son séjour prolongé dans certains organes, le foie et le cerveau, par exemple. Ainsi l’alcöol ne se détruirait pas dans l’organisme, par conséquent ne pourrait devenir une source de chaleur, pas plus qu’un aliment, puisque une caractère essentiel de ce dernier est de ne pouvoir être utilisé pour la nutrition sans subir une métamorphose. Il est des circonstances anormales où l’alcöol se décompose partiellement dans le tube digestif; ainsi, par exemple, à la suite de libations copieuses accompagnant un bon repas, l’alcöol peut, en présence des matières azotées alimentaires et de l’air ingérés, donner lieu à de l’acide acétique” (p. 216).

We doubt not that this picture of the use, or rather abuse, of alcohol will be pleasing to our friends the total abstainers.

Dr. E. Smith, too, thinks that alcohol does not increase the production of heat in the body, except so far as it acts by increasing the activity of the vital functions. He states:

“Thus, on a review of the experiments with moderate or small doses of these strong alcohols properly diluted, and taken on an empty stomach, it is shown that the vital actions are generally increased with pure spirits of wine and rum, whilst they are lessened with brandy, and greatly lessened with gin. Whisky varied more than the other alcohols, but generally its tendency was to lessen vital actions. But it is needful to repeat the observation that there is much greater disturbing influences excited by these agents than by ordinary foods, and there was not that regular progression of increase or decrease usually observed with other agents. This extended even to the rate of pulsation and respiration” (p. 386).

The experiments of M. Baudot, and the still more recent ones of Parkes and Wollowicz, and of Dupré, have thrown considerable light upon the uses to which alcohol is put in the human economy. The destination of alcohol, which forms so large a constituent of the ordinary drink of ordinary Englishmen, cannot, however, be said as yet to be satisfactorily determined. Lallemand’s statement that alcohol is not oxidised in the body, but is excreted unchanged, may be said, nevertheless, to be completely refuted. Indeed, it may well be doubted whether any article of food or drink, or a drug, exerting a decided physiological or therapeutical effect, can be excreted unchanged. The assumption that there is such an elimination strikes at the very root of the doctrine of the non-creatability of any form of force, as laid down in the doctrine of the conservation of energy.



*Ex nihilo nihil fit* is true in dietary as in other matters, and we know but little of the forms in which drugs are excreted. The whole subject of the ingestion and assimilation of alcohol is one which will, we believe, amply repay the experimenter, and is one which needs the investigation of one alike free from the prejudices of those who advocate total abstention and those who urge the use of alcohol by persons in ordinary health. We agree with Dr. Pavy that the subject is open to investigation by both the physiologist and the chemist alike. We need also the investigation of alcohol not only *per se*, but also of the homologues of alcohol, as *e.g.* fusel oil; and we imagine that the observation of the effects of the higher homologues of alcohol, as well as of that of its lower homologue—methyl alcohol—will throw new and interesting light upon the effects and uses of ordinary alcohol. In a word, we need the investigation of the physiological effects of *the alcohols as a class*.

Before we dismiss the consideration of alcoholic beverages we cannot do better than quote some valuable remarks on the use and abuse not only of alcoholic, but of other beverages at meals; believing, as we do, that errors in this respect are most rife, especially in what is, *par excellence*, termed “Society.” Dr. Pavy says:

“If a plain and wholesome drink be drunk, the error is not likely to be committed of taking too much. After compensating for the loss by the skin, and with the breath, the surplus passes off through the urinary channel, and it is desirable that this surplus should amply suffice to carry off the effete products forming the solid matter of the urine in a thoroughly dissolved state. The notion has been started that it is advisable to restrict the amount of fluid taken with the meals with the view of avoiding the dilution of the gastric juice. Whether, as the result of the influence of this notion upon the public mind or not, mischief, I believe, is frequently occasioned, especially amongst the higher ranks of society, by a too limited consumption of fluid. Instead of taking a draught of some innocent and simple beverage, it is at many tables the fashion to sip fluid—and this a more or less strongly alcoholic one—only from the wine-glass. It is a mistaken notion to think that when we drink with a meal we are diluting the gastric juice. The act of secretion is excited by the arrival of the meal in the stomach, and the gastric fluid is not there at the time of ingestion. It happens, indeed, that the absorption of fluid takes place with great activity, and the liquid which is drunk during a meal becoming absorbed may be looked upon as proving advantageous by afterwards contributing to yield the gastric juice which is required” (p. 313).

It is not often that we see the dyspeptic drinking copious draughts of plain water, aerated waters, mild table-beer, or well-diluted claret, with their meals.

Space forbids us to do more than refer to the immense benefit that has resulted to the British community by the enforcement of the last Adulteration Act. That measure which passed through Parliament, much to the astonishment of its opponents, is in some respects a crude and imperfect measure, as, indeed, all amateur legislation is apt to be; nevertheless, it possesses too many valuable provisions to admit of its being lightly repealed. Doubtless the deliberations of the late Select Committee of the House of Commons will lead to some modifications of the provisions of the statute; but we imagine that the alterations will not be great. We believe that the horrors raised by our contemporary 'The Grocer,' and other trade organs, against the iniquity of the existing law, are a measure rather of the just severity with which the enactment presses upon the trading portion of the community than of the hardships to which those gentlemen are subjected.

If we were asked to specify the benefits which have hitherto resulted from the Act, we should state that the admixture of alum with bread is rapidly becoming a thing of the past; that the filthy refuse which formerly passed under the name of tea can no longer be palmed off upon a too credulous public anxious to buy "cheap" provisions; that green tea can now be bought unfaced; that our milk is, to say the least, less watered; that the admixture of suet, &c., with butter is less frequent; that pepper can be bought pure nearly everywhere, and so can mustard, if asked for; that it is usual now-a-days for the grocer to ask his customers whether they prefer pure or mixed coffee; and that the adulteration of beer is now chiefly confined to mixture with that innocent liquid, water. We wish we could say that the Act is anything more than a dead letter with respect to drugs. But the consideration of these is beside our present theme.

One remarkable and perhaps unlooked for result is, that but few adulterations have been detected positively injurious to health. Indeed, with the exception of alum in bread, scarcely any adulteration injurious to health has been brought to light. The modern adulterator is too wary and skilful to mix his wares with positively hurtful substances, and wisely contents himself with such admixtures as simply increase weight or bulk, or please the eye of the customer.

The work of Dr. Cyr appears to us to treat of dietary in a somewhat hasty and superficial manner, notwithstanding that the author displays a considerable acquaintance with the literature of the subject. His volume, though doubtless useful, possesses but few attractions for English readers. Of dietary, Dr. Edward Smith does not pretend to treat, and his volume



professes to be exclusively devoted to the subject of foods. As it forms one of the so-called International Scientific Series, which purports to be a Series of Popular Treatises intended for the use of the "non-scientific" public, treating the latest investigations in the various departments of science in an explanatory manner and with freedom from technicalities, we were disappointed to find the book full of technicalities, and, we should imagine, anything but pleasant reading to a "non-scientific" reader. Many parts of the book will be incomprehensible to those who are not familiar with the terms "grains," "grammes," "cu. in.," "cub. cent.," "grs.," "c.c." It would surely have been better to have reduced the weights and measures to one uniform standard, and to have dispensed with such abbreviations by printing the designations in full. The long, intricate, and uninteresting recipes for various dishes for the table, with which the book is swelled out, might also well have been omitted. The amount of valuable material, more especially the *résumé* which the author gives of his own researches, will, we imagine, render the volume more interesting to the scientific than to the non-scientific reader.

Dr. Pavy's 'Treatise on Food and Dietetics' is a more ambitious and bulky volume than either of the others. We have quoted sufficiently from it to enable our readers to judge of its general character. We doubt, however, whether it really contains more useful matter than the smaller and less pretentious work of Dr. Smith. The chapters from which we have so largely quoted on the physiological properties of foods are excellent. This makes us the more regret that the chapters on dietary are, when closely examined, found to be so meagre and unsatisfactory. A classical English work on dietary remains to be written. Perhaps the subject is as yet hardly sufficiently advanced to permit of its being systematically treated of in a satisfactory manner. In any future edition of his book we should recommend the author to reduce its bulk to one half by the omission of all superfluous padding. We refer to the long, intricate, ancient, and perfectly useless tabular analyses with which the book is profusely interlarded. Of what interest or use, we may ask, is a long analysis showing the composition of, say bilberries or artichokes? and yet such analyses as these—mere compilations—form no inconsiderable portion of the letter-press of the work, to the exclusion of much interesting and valuable matter that might well have been inserted. Notwithstanding these defects, we can cordially recommend the book to our readers. We may add, that the proof-sheets appear to have been very carefully corrected, and that we have discovered but few errors in statements of facts.

IV.—The Puerperal Diseases.<sup>1</sup>

THIS handsome volume is a further proof of the energy with which our American brethren cultivate obstetrics, the immense importance of which, as a branch of scientific medicine, they obviously appreciate more justly than is as yet the case in this country. Many of our most distinguished physicians and surgeons will doubtless enjoy a hearty laugh at the following sentence from Dr. Barker's preface, which seems so absurdly opposed to our prevalent notions about the obstetric branch of medicine. We should be sorry to baulk them of this enjoyment, and we therefore quote it, only remarking that though it may seem to them an amusing piece of American bunkum, it contains a large substratum of truth, which, ere long, will make itself surely felt, in spite of the conservative opposition which has yet to be overcome:—

“At the present day, for the first time in the history of the world, the obstetric department seems to be assuming its proper position as the highest branch of medicine, if its rank be graded by its importance to society, or by the intellectual culture and ability required, as compared with that demanded by the physician or surgeon. A man may become eminent as a physician, and yet know very little of obstetrics; or he may be a successful and distinguished surgeon, and be quite ignorant of even the rudiments of obstetrics. But no one can be a really able obstetrician unless he be both physician and surgeon; and, as the greater includes the less, obstetrics should rank as the highest department of our profession.”

Dr. Barker has been peculiarly fortunate in his subject. There is no part of midwifery more important, and none which more frequently calls for all the knowledge and experience of the practitioner, and yet there is none about which, as a rule, he is more ignorant. The reasons for this are obvious. In the first place, on account of the absurd regulations of the Medical Council, which compress the study of midwifery into a short three months' course, it is manifestly impossible for the teacher even to attempt to give the most elementary instruction on the subject. Nine out of ten of our students, therefore, leave their schools almost absolutely ignorant of the topics which this book discusses. Nor is it only that opportunities for theoretical instruction are wanting, for the same may be said with regard to practical knowledge, in consequence of the absence of large obstetric clinics like those of Vienna, or of the Bellevue Hospital itself.

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<sup>1</sup> *The Puerperal Diseases. Clinical Lectures, delivered at Bellevue Hospital, New York.* By FORDYCE BARKER, M.D. London, 1874.



Then, again, the recent advances of science with regard to many of the subjects have been so great that the information contained in most of our standard works of reference may be said to be obsolete, and, with the exception of Dr. Leishman's work, there has been no attempt to fill the blank. By devoting a special work to the diseases of the puerperal state, Dr. Barker has been enabled to discuss them with a fulness and at a length which is impossible in a system of midwifery, and the result is certainly the production of a work which is not only highly creditable to the author, but which we can very strongly recommend to the serious study of the practitioner. He will find in it much knowledge which he can obtain nowhere else, which will help him out of many difficulties, and relieve him from much anxiety.

We cannot, of course, pretend to review the whole work, but we shall attempt to give our readers a general survey of its more important contents.

The first chapter treats of ordinary puerperal convalescence, including retention of urine, after-pains, and secondary hæmorrhage. With regard to after-pains, which are frequently the source of so much suffering that women often dread them almost more than labour itself, they are properly referred to their true cause,—the retention of coagula in utero, and the possibility of preventing them altogether, by securing proper contraction of the uterus after delivery and the expulsion of coagula at the same time, is duly insisted on. There can be no doubt that they can almost always be prevented if due care be taken in this way. Certainly the administration of a large dose of ergot immediately after the birth of the child, which is recommended as an occasional expedient, might very properly be made a routine practice. It is one which we have invariably adopted for many years with great advantage. If this be done, and if pressure on the uterus be kept up regularly for twenty minutes to half an hour after the expulsion of the placenta, both post-partum hæmorrhage and severe after-pains, each of which depends on the same cause, may almost certainly be prevented. It is curious how both of them are so much more common in the practice of some medical men than in others; and we believe that they will occur in exact proportion to the amount of care that is devoted to the production of efficient tonic uterine contraction. Whether there be a second variety of after-pains, such as Dr. Barker describes as being of purely neuralgic character, coming on a few days after labour, associated with a firmly contracted uterus, and to be relieved by the administration of large doses of quinine, and by chloroform liniments locally applied, we cannot say. Certainly the description

does not correspond with anything with which we are ourselves familiar.

Secondary post-partum hæmorrhage, which is treated of at some length in this chapter, is of considerable importance and interest, inasmuch as it is not discussed satisfactorily in any of our standard works. Almost all our knowledge of the subject is derived from Dr. M'Clintock's excellent paper, which, however, is by no means so generally known as it ought to be. We observe that Dr. Barker recommends the injection of the solution of per-sulphate of iron in secondary post-partum hæmorrhage due to uterine relaxation rather more freely than we should be disposed to do. Our objection is not founded on any hypothetical risk of the injection itself, which we believe to be mainly imaginary, but because even a few days after delivery the os is likely to be closed and contracted, and under such circumstances every intra-uterine injection is apt to be hurtful. The local astringent, therefore, would be more safely, and equally effectually applied, by swabbing out the cavity of the uterus, as in the non-pregnant state. If the injection itself is used, we should at least first satisfy ourselves that the os is widely patulous.

Among the local causes of secondary post-partum hæmorrhage, retroflexion of the uterus holds a not altogether unimportant place, and its absence should be carefully ascertained. To this we observe that Dr. Barker makes no allusion.

The next chapter treats of the diet of the lying-in women, in which the views now pretty universally adopted in this country are maintained. How the absurd practice of starving the puerperal patient into debility, and treating a perfectly normal function as if it were a disease, came to be so prevalent it is not easy to understand; certain it is that the starvation theory when once abandoned will never be resumed. It is well, however, we think, to rely mainly on the inclination of the patient as the safest guide. If the woman has an invincible repugnance to solid food, no advantage is likely to accrue by forcing it upon her, and in such a case milk, and beef or chicken tea, will answer every purpose. But if she is inclined for more substantial food there is no conceivable physiological reason why she should not have it, only remembering that as the patient is in the recumbent position, and debarred from exercise, it ought to be of a light and easily assimilated character.

The remainder of the chapter contains a valuable dissertation on hæmorrhoids in pregnancy and the puerperal state, which contains a very novel view of their management, and will be read with much interest and instruction. So far from considering



aloetic purgatives as contra-indicated, he looks upon them as of extreme value in their treatment, whether they are associated with constipation or diarrhœa. In either case he gives from half a grain to a grain of the powdered aloes, night and morning, in the form of a pill, but adds, when the latter is present, a small quantity of opium to each dose. Castor oil, so favourite a laxative in these cases, he regards as irritating and injurious. In one or two instances in which we have recently adopted this medication we have certainly found it useful. When hæmorrhoids are of any size during labour he recommends the forcible distension of the sphincter and immediately after the child is born, the patient being previously anæsthetised. The cases, however, are certainly comparatively rare which are sufficiently severe to justify this somewhat heroic proceeding.

The succeeding chapter treats of the management of the perinæum during labour. The injurious effects of the so-called "supporting the perinæum" have not unnaturally led to the opinion entertained by Hewitt and other writers that it is safer to leave the perinæum alone, but this opinion we quite agree with Dr Barker is in itself erroneous. We have no doubt whatever that an intelligently employed management of the perinæum during its distension will materially lessen the risk of laceration and facilitate delivery. But it must be distinctly understood that what we have to do is not to mechanically prevent rupture by hard pressure, but to favour relaxation of its structures, and to direct the head forward towards the pubes in the last stage of delivery.

Dr. Goodell's method herein described, which consists in relaxing the perinæum by passing one or two fingers of the left hand into the rectum and hooking forward the sphincter, is based on precisely this principle. We have occasionally tried it, and found it answer well, but it is disagreeable to both patient and practitioner, and the same end can be equally well gained by other means. Dr. Barker makes no allusion to surgical treatment beyond recommending lateral incision when rupture seems imminent. It would have improved his paper had he written on the importance of applying one or two sutures immediately after delivery, when even moderate laceration has occurred. This almost always insures adhesion, and may prevent the necessity of a secondary operation; whereas, if the insertion of the sutures be postponed for even twenty-four hours, they are apt to prove useless.

The chapters on Albuminuria and Eclampsia will be read with interest, and form an excellent monograph on this difficult subject. It must be confessed that, in spite of all that has been written upon it, we are still very much in the dark as to the true

pathology of this terrible complication of labour. Dr. Barker is inclined to take the view, which is apparently gaining ground, that there is not the intimate connection between uræmia and eclampsia that has been generally supposed, or at least that there are a much larger number of cases than we have been in the habit of believing, which are altogether independent of uræmic poisoning. He considers the experiments of Hammond as conclusive proof of the incorrectness of Frerich's theory of the conversion of urea into carbonate of ammonia, and the presence of the latter as the toxic element. He makes no allusion, however, to the more recent counter-experiments of Spiegelberg, which would seem to show the correctness of Frerich's hypothesis.

With regard to the cases in which convulsions occur before albuminuria has developed itself, and in which the latter only comes on some time after their onset, he quotes from Braxton Hicks's excellent paper on this point, and evidently agrees with him that they are far from uncommon. He refers to Frankenhauser's researches on the nerves of the uterus, and the intimate connection they show to exist between the uterine and renal plexuses, as rendering it probable that this connection may, in some way not yet worked out, explain the true pathology of the disease. He also describes the view of Rosenstein, which refers the eclampsia to acute cerebral anæmia, produced by tension of the aortic system, combined with an impoverished and hydræmic state of the blood, which, on its part, produces œdema of the brain-substance, and consequent mechanical pressure on the blood-vessels.

This theory is certainly ingenious, and affords, perhaps, the best explanation of this difficult subject. It is the one adopted by Schroeder in his recent work. At any rate, the evidence that eclampsia is not necessarily uræmic seems to be daily increasing in strength. With regard to treatment, Dr. Barker's directions are clear and intelligible. He favours, however, the abstraction of blood more than is customary in this country, although even with us it has always found advocates. Personally we have never looked upon it with favour, and have found that in chloroform anæsthesia and the other plans usually recommended, we had agents which always seemed safer and more reliable. We think Schroeder puts the merits and demerits of the question better than any one else, and his view affords a satisfactory explanation of the varying opinions entertained with regard to it. Assuming the correctness of Rosenstein's theory, he points out that venesection, by suddenly diminishing the blood-pressure in the brain, in consequence of vascular depletion, may temporarily diminish and even arrest the attack ;



but very shortly after the cerebral tension becomes as great as before, owing to the blood being the more impoverished. "Experience," he says, "speaks in favour of this opinion. Venesection has often given favorable and exceedingly rapid results, but frequently the attacks have soon recurred, and have taken a far more unfavorable course." On the whole, therefore, there seems grave theoretical grounds for considering venesection to be a somewhat dangerous and double-edged weapon, which should at any rate be reserved for very severe and intense cases, where an immediate effect is essential.

Dr. Barker places very great reliance on chloroform, as all do who have experienced its benefit. We are somewhat surprised to find that he speaks with disfavour of chloral hydrate, which is contrary to our own experience, as we have seen it act decidedly well. It has the advantage of acting more continuously than chloroform, which, however, may be with equal utility employed in combination with it.

Passing over a series of chapters on affections of the breasts connected with the puerperal state, we come to those on Puerperal Mania. The frequency with which this disease occurs in America strikes us as very curious. Dr. Barker estimates the ratio of puerperal mania to the whole number of cases of labour in the Bellevue Hospital as 1 in 80. This certainly contrasts in the most remarkable manner with European statistics, which indicate its frequency to vary from 1 in several hundreds to 1 in 1228, which is the estimate arrived at by Scanzoni. This striking difference Barker attributes to the much larger number of unmarried women, in whom mental causes are very prominent, who are delivered in the wards of the hospital, as well as to climatic causes, which he believes to have a not unimportant influence.

We observe that Dr. Barker entirely disagrees with the theory which was first broached by Sir James Simpson, and has since been maintained by Donkin and others, that puerperal mania is generally dependent on some septic condition of the blood, and is almost always associated with albuminuria.

He says that he has been on the constant watch for albuminuria, and has found it associated with so small a number of cases, that he is compelled to regard it, when present, as a coincidence, and not a cause. The subject requires a much more extended study than it has yet received, and possibly we may find that some toxic condition other than uræmia is present. For our own part we have always seen considerable difficulties in the way of accepting this theory. Amongst these is the fact admitted by Simpson that the albuminuria is merely transient and disappears in a day or two, while its supposed effects

are always observed over a lengthened period. The explanation given of this fact, that when once the disease is started it proceeds of itself, is purely hypothetical, and based on no reliable grounds. Besides, for all we know, transient albuminuria may be much more common after delivery than is generally supposed. Many distinguished alienist physicians amongst us entirely disbelieve this theory, and, on the whole, we must consider it as an ingenious but unproved hypothesis.

The chapter on Phlegmasia Dolens is hardly satisfactory. It gives us, indeed, a very characteristic and accurate account of the disease and its symptoms; but when we arrive at that part of it which treats of its pathology, we find that though all the theories broached in explanation of it are successively demolished, we are, at the end, left as much in the dark as ever as to its real nature. Thus, Dr. Barker proves to his own satisfaction that it is not crural phlebitis, or lymphangitis, or anything else that it has been supposed to be, and that, amongst modern writers, Mackenzie, Fox, Simpson, are all more or less in error in their views, but he then leaves us with the comfortless assurance that "we are still as ignorant of its real pathological nature as we are of that of rheumatism and many other diseases."

That there is much to learn about the intimate nature of the disease no one will deny, but we can hardly agree with Dr. Barker that we are altogether so ignorant with regard to it as he believes. We hold that the explanation of the obscurity which has prevailed about this disease is, that on account of its evident local manifestation, it has always been studied as a special and distinct affection, and that thus many points as to its etiology and pathology have been lost sight of. It is impossible within the narrow limits of a review to enter into this complex subject at length. It seems to us, however, that the weight of evidence goes strongly to prove that phlegmasia dolens is only one of the local manifestations of a general blood dyscrasia, the tendency of which is to produce thrombosis; and that it is this disease, puerperal thrombosis, and not phlegmasia dolens, *per se*, that requires study. The fault, in which Dr. Barker partakes, has been, that authors have invariably discussed it as a distinct and separate affection. He gives us, for the first time in any English work on puerperal disease, an excellent chapter on Puerperal Thrombosis and Embolism, to which we shall presently refer; but he considers these as altogether distinct from phlegmasia dolens. Now, is there, in fact, any justification for this separation? Is there not, on the contrary, a close similarity in their histories which show them to be really separate manifestations of the same state? Their origin, the class of patients they attack,



the pathological changes observed on post-mortem examinations, and indeed their whole history, prove them to be analogous. The difference is chiefly in the vessels affected, whence arise results apparently so different, that it is difficult, at first, to believe that they depend on a similar cause. Nothing can, indeed, be more distinct from the fearfully sudden death of cardiac thrombosis, or the mortal agony of the struggle for breath that is associated with it in cases not immediately fatal, than the slow progress of the swelling in phlegmasia dolens. And yet, when the cases are carefully analysed, the points of similarity between the two are striking and remarkable. It is certainly true that there is probably something beyond mere peripheral thrombosis in the venous system of the lower extremity required to produce the peculiar tense, shiny, and inelastic swelling so characteristic of the disease. Whether this be due, as the researches of Tilbury Fox would tend to show, to some concomitant affection in the lymphatic system we cannot certainly say. There is no doubt much force in Dr. Fox's arguments, and it must, we think, be conceded, that obstruction of the veins, *per se*, is not of itself sufficient to produce the peculiar appearance of the limb. There is no inherent improbability in the supposition that the same morbid state of the blood which produces thrombosis in the veins, may also give rise to such an amount of irritation in the lymphatics as may interfere with their function, and even obstruct them altogether. But the essential and all-important point in the pathology of the disease, as admitted on all hands, seems to be the thrombi in the vessels; and the probability of there being some, as yet undetermined, changes in addition thereto, by no means militates against the view we hold of the intimate connection of phlegmasia dolens with the other results of thrombosis in more distant vessels. If, then, writers would talk of phlegmasia dolens as a manifestation of peripheral thrombosis, and study it in connection with thrombosis in other parts of the body, we believe that a much more accurate and practical view of the whole subject would be taken.

The succeeding chapter on Puerperal Thrombosis and Embolism gives a very excellent *résumé* of all that is known on this interesting subject, so strangely overlooked in our systematic works, and yet of such immense importance to the obstetrician. It is satisfactory to find that Dr. Barker admits the possibility, not only of spontaneous thrombosis of the right side of the heart and pulmonary arteries, which is so strongly denied by Ball, Bertin, and other writers on the subject, but also fully recognises the fact that even the most formidable symptoms of pulmonary obstruction may be recovered from by the absorption

of the deposited thrombi, and that thus are explained many of the cases of great danger and obscurity which are every now and then occurring.

In a work on the Puerperal Diseases we, of course, considering their immense importance to obstetrician and patient, naturally expect to find great attention paid to those formidable complications of delivery known by the generic name of puerperal fever. It must be confessed, however, that Dr. Barker has been no more fortunate than other writers in simplifying and rendering intelligible the complex subject. Nothing can be more disheartening and bewildering to the student than the endeavour to unravel and understand this obstetric puzzle. When we have one class of practitioners referring all cases to certain local inflammations, others to traumatism and septic absorptions, some, again, to the prevalence of a specific and peculiar fever, it is hardly a matter of astonishment that we find that practitioners, as a rule, have so hazy an idea as to what the dreaded puerperal fever really means.

No better evidence can be given of the extreme diversity of opinion on this subject than the excellent *résumé* which Dr. Barker gives of the various prevalent theories of the disease :

These are, 1st, the theory of the localists, who believe that the disease consists essentially of inflammation of some of the organs or tissues connected with parturition, leading to secondary general symptoms.

2nd. The septicæmic theory, much in vogue in Germany, which attributes all cases to the absorption of decomposing organic matter from traumatic lesions about the uterus or its cervix.

3rd. Those who believe that the disease is due to an essential and specific fever, peculiar to the puerperal state, which is as much a distinct disease as typhus, typhoid, or measles. This is the theory which Dr. Barker himself adopts.

4th. Those who include under the terms puerperal fever all the zymotic diseases, such as scarlatina, erysipelas, septicæmia, and all the severe primary inflammations when they occur in a puerperal woman. "This class does not reject the idea of a primary vitiation of the blood, but terms the disease a puerperal fever, whatever may be the specific nature of the primary poison."

5th. The theory of Professor Martin, of Berlin, "that the diphtheritis in the genitals of lying-in women is the only essential element of puerperal fever."

6th. The theory of Hervieux, that there is a peculiar puerperal miasm, which engenders phlebitis, metritis, and a multiplicity of puerperal diseases.



Well may the intelligent layman who chances to read this formidable catalogue of theories complain of the uncertainty of medical science. Truly we want an obstetric Jenner, who will clear up for us this chaos, and teach distinctly what we are to believe.

For ourselves, we are bound to state that the fourth theory in this list, which is that held by the majority of British obstetricians, in spite of its indefiniteness, seems best to explain the protean forms of puerperal disease.

We entirely fail to see that Dr. Barker has proved his own view as to the existence of a special form of fever as distinct as typhus or typhoid. He says, indeed, that he is not called on to do so, as "it must be remembered that there are no pathognomic symptoms of any of the essential diseases, with the exception of the exanthemata, and these can hardly be called exceptions." But surely this is a most untenable statement. Every student is called upon to know how to distinguish typhus, typhoid, or relapsing fevers, which, in spite of their variations, have a sufficiently clear and distinct type. But any one reading Dr. Barker's description of the so-called essential fever must entirely fail to see anything in the symptoms which distinguish its features from those accompanying what he believes to be the entirely distinct conditions of puerperal septicæmia, and other allied affections. Nor does it seem possible to doubt that the contagion of distinct specific maladies, such as scarlatina or erysipelas, conveyed to the puerperal patient, produces in her symptoms altogether wanting the peculiarities of the parent disease, but very similar to those of the supposed specific puerperal fever.

It seems to us impossible to read Dr. Hicks's most instructive paper in the 12th volume of the 'Transactions' of the Obstetrical Society without admitting this fact. He there collects eighty-nine cases in consultation practice, most of them presenting the phenomena of what is usually known as puerperal fever, many of which he was able to trace to various zymotic diseases, the contagion of which had too often been conveyed to the patient.

Another valuable paper, in the same 'Transactions,' gives an account of the disastrous experiments tried in King's College Hospital, of having a lying-in ward within the walls of a general hospital. It is there related how, when epidemics of distinct and specific zymotic diseases were rife in the hospital, such as erysipelas, puerperal fever carried off the lying-in patients. These showed no symptoms of erysipelas, but who could doubt that they were caused by the same specific miasm? This view seems not very different from that of Hervieux, who believes in a peculiar

puerperal miasm, and it admits of the septic origin of many cases from the absorption of decomposing organic material. Of the reality of this autogenetic form of puerperal disease there can be no reasonable doubt. On the whole, then, it seems to us that, in spite of Barker's objections, the theory generally held in this country is the most catholic in its scope, and the best fitted to explain the remarkable diversities of opinion held on the subject.

As regards the treatment of these puerperal affections, Dr. Barker's remarks seem to us highly judicious, and well worthy of careful study. They are far in advance of the indefinite directions contained in most of our works, and show evidence of much careful clinical study. The point which will most strike the British practitioner is the author's evident partiality for the *veratrum viride*, as a means of lowering a rapid circulation, and bringing the pulse down to a normal standard. This and the similar use of tincture of aconite for the like purpose, is obviously much used in America, although the plan has never made much way in this country. In the cases in which we have ourselves employed aconite for this purpose we have had good reason to be highly satisfied with its results. It is certain that these drugs should have a more extended and careful trial than has hitherto been given them.

In conclusion, we can strongly recommend this treatise to our readers. They will never regret any study they may give it, for it contains the fruit of much honest and laborious work.

## V.—The Use of Water in Chronic and Acute Diseases.<sup>1</sup>

It is with some hesitation that we venture to approach the subject of the water-cure, or hydropathy, so little encouragement has it hitherto received at the hands of English physicians; but

<sup>1</sup> 1. *Traité théorique et pratique d'Hydrothérapie.* Par le Dr. BENI-BARDE. 1874.

2. *Nouveau Dictionnaire de Médecine et de Chirurgie pratiques.* Tome xviii, 1874. Art. "L'Hydrothérapie."

3. *Hydropathy as applied to Acute Diseases.* By T. R. ARMITAGE, M.B. London, 1852.

4. *Deutsches Archiv für Klinische Medecin.* Vol. I. LIEBERMEISTER, "Ueber Febrilen Temperatursteigerungen."

5. *Id.*, Vol. IV. LIEBERMEISTER, "Bericht über die Resultate der Behandlung des Abdominaltyphus im Spital zu Basel."

6. *Id.*, Vol. VIII. G. MERKEL, "Die Resultate der Kaltwasserbehandlung des Typhus im städtischen Krankenhause zu Nürnberg während des Jahres 1869."

7. W. LEUBE, "Mittheilungen über die Typhusepidemie in der Festung



as the works before us testify this therapeutic method has been more widely supported on the Continent, and we are rightly called upon to give it a careful consideration before rejecting it as useless. We are not however prepared to raise it to the dignity of a system, founded on any one principle in therapeutics, but rather look upon it as a section of that art, arbitrarily separated, and dealing with remedies very different in their action upon the economy, but grouped together on purely physical grounds, viz. the fact that they can be applied by means of water with certainty and discrimination. These agents and their effects we shall consider further, and would only remark that, if the early promoters of hydropathy have injured their cause by claiming success in diseases known to be incurable, or by showing themselves incapable of accurate and trustworthy diagnosis, this method may still present for certain cases means of treatment which are more efficacious and at the same time not available in ordinary practice. It must be allowed that by the various methods of hydropathy, we are afforded an opportunity of so acting upon the skin as to exert a powerful influence on the systems of which it forms part, and the functions to which it contributes: indeed, many such methods are recognised therapeutic agents, and belong as much to so-called rational medicine as to hydropathy, but the difficulties in their practical application must hinder the systematic study of their effects under any other condition than those presented by establishments specially constructed for the purpose. It thus happens that the patient must be brought to the treatment, and not the treatment to the patient; and the cases being accumulated under the care of the director, he becomes almost of necessity a specialist, with the tendency to magnify the importance of his subject; a fault which peculiarly attaches to the physician of this or that organ, and the advocate of this or that "cure."

The fair spirit and moderate tone in which the work of Dr. Beni-Barde is written, and the full descriptions it contains of the diseases treated, will show that he has made an attempt to avoid the errors above alluded to; but it occasionally appears

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*Ulm im Winter 1870—71, und über ihre Behandlung im dortigen Barakenspitale."*

8. BÖHM und MICHEL, "*Beobachtungen über die Kaltwasserbehandlung des Abdominal Typhus in Kriege.*"

9. *Id.*, Vol. IX. SCHOLZ, "*Bericht über die Resultate der Kaltwasserbehandlung des Unterleibstyphus in der Krankenanstalt zu Bremen.*"

10. F. RIEGEL, "*Ueber die Resultate der Kaltwasserbehandlung der Unterleibstyphus im K. Julius Hospitale zu Würzburg im Jahre, 1870 und '71.*"

11. '*Prager Vierteljahrschrift.*' Vol. 113, 1872. GÖLZ. '*Beobachtungen und Erfahrungen über die Anwendung des Kaltenwassers beim Typhus.*'

that the diagnosis has not been well founded, though the treatment has been successful. The article "*l'Hydrothérapie*," in the "*Nouveau Dictionnaire de Médecine et de Chirurgie pratiques*," is an abridged edition of the first portion of the same author's treatise, describing the principles and methods of application, and we shall therefore refer entirely to the principal work named for information as to the present position of this department of therapeutics. The range of usefulness attributed to the method is considerable, but in some directions sharply defined. The statement that "hydropathy is the medication of chronic complaints, and is only exceptionally suitable for acute diseases" is frequently insisted on in different places and at different times; yet the number of chronic diseases amenable to this mode of treatment is naturally further reduced by the exclusion of most cases of new growths, and other modifications in the normal state of the tissues, as inflammations, hypertrophy, atrophy, and degeneration; and of structural change of the various organs, though in this last class success in relieving symptoms may sometimes be attained. The diseases thus left for consideration are chiefly, cachexiæ, diatheses, nervous diseases, and functional disorders; and to these groups, with one comprising affections of the locomotor apparatus, Dr. Beni-Barde devotes a large portion of the clinical part of his work. But we can scarcely review the uses and effects of hydrotherapeutics before we have inquired into the principles on which it pretends to be founded, as well as the effects which its various methods have upon the economy. And firstly, water in the form of bath and douche is seldom used merely as water, but in nearly all cases it serves as a means of submitting the body to regulated degrees of heat and cold. These indeed are the two active agents whose therapeutical effects form the study of this department of the medical art; and accordingly we find among the processes included a few in which water as an external application takes no part, but the required temperature is reached by other means; the dry pack, the hot-air bath, and the Turkish bath, in the essential part of the operation, are instances of this. They are not, however, so frequently employed as hot and cold baths and douches, the wet pack, fomentations, wet compresses, lotions, and affusions, the influence of which may be different according to the extent of the body submitted to the treatment, and the length of time occupied in its application.

It will not be necessary to do more than enumerate the effects of cold and heat, both locally and generally. Acting upon the skin with which it comes in contact, cold produces a contraction of the muscular fibre resulting in cutis anserina, pallor of the surface from narrowing of the small vessels, and



consequent diminution of the quantity of blood in the part ; and, thirdly, after prolonged application, numbness and anæsthesia from its action on the sensory nerves. This depression of function may affect the whole body, if the extent, duration, and intensity of the cold be sufficiently great ; or, on the other hand, a tonic or stimulating effect may be obtained by slight degrees employed for shorter periods. For all these purposes it may be used, as well as with the sole object of diminishing the temperature of the body, when, as in pyrexia, an unusual amount of heat is produced ; in the latter case the effect of cold will be different from that previously described, since it is not acting upon the healthy body, but upon the system already disturbed in its working by the morbid influence of a high temperature. Only by a considerable abstraction of heat can this be reduced within the limits of health, and it is not until these are passed that the system can be properly said to be subjected to the influence of cold. Of this antipyretic influence, however, we wish to make some remarks farther on, since in accordance with the limitation of the usefulness of hydropathy mentioned above, it has not in Beni-Barde's work received the consideration to which it is rightly entitled.

The effects of a moderate heat are, on the whole, the reverse of those of cold ; relaxation of muscle soon follows upon the first slight contraction, and leads to dilatation of arterioles and redness of the surface ; the sudoriparous glands are at the same time stimulated, and perspiration takes place. If this be free, it may cause great depression ; but, quite apart from this, the mere elevation of the temperature of the various parts of the body can be shown to impair their functions to a considerable and even dangerous degree. The therapeutical effects to be obtained from cold and heat are thus considerable, namely, hæmostatic, antiphlogistic, tonic, stimulant, sedative, sudorific, and revulsive, and the particular methods by which these ends are obtained deserve a word before we go farther into the merits of the treatment.

Of these methods we need say little of the use of cold water and ice for repressing hæmorrhage and preventing inflammation, of the employment of local anæsthetics, and of the application of warmth and moisture for the relief of pain, for their value is recognised by all. The more strictly hydropathic means are the bath, the douche, and the pack. The last of these may be administered as dry pack or wet pack, the former having a direct stimulant and heating effect, and resulting in free perspiration ; in the latter this excitant effect may be delayed, since the envelopment of the body in the moist cloths has a cooling, sedative influence, which is greater and more pro-

longed, the more moist the cloths are. When the packing has become warmed by the skin it aids in retaining the heat, and hence the final result is similar to that of the dry pack, namely, one of stimulation; however, by changing the packing, this secondary effect may be avoided, and the sedative influence maintained. Vapour baths, hot-air baths, Turkish and Russian baths, are so many other means of procuring perspiration, most beneficial when followed immediately by cold immersion or douche, and never to be recommended when the nervous system is too readily excitable; powerful rubefacient effects also can be obtained from the successive application of hot and cold water, the reddening produced by the hot douche being much augmented by subsequent application of cold water. A most efficient revulsive of this class, called the Scotch douche, consists in a hot douche, the temperature of which is slowly and progressively raised from 86° to 112° Fahr., followed immediately by a short application of absolutely cold water. Lumbago, torticollis, and sciatica, are usefully treated by this means. But it is to cold water alone that hydropathists owe their most successful cures; and this is most often applied in the form of douche, the effect varying with the temperature of the water, the duration of the application, and the force with which it is directed against the part of the body concerned; thus the most stimulant effects are obtained by a cold short douche, directed with some force, and a less exciting effect by a warmer application, used with less force, but for a longer time. Sedative effects are obtained by continued cold, or even with water of moderate temperature, if continuously applied. Somewhat similar results follow the use of general baths, sitz baths, and foot baths when judiciously employed; friction with wet cloths may be made to have exciting or sedative effects, according as the cloths are drier and more forcibly rubbed in, on the one hand, or damper and more lightly applied, on the other; and, lastly, douches have generally a stimulant effect, can be applied to any part of the body, and have received different names from the nature of the nose or nozzle through which the water flows.

The shower bath and the movable douche (*douche en jet mobile*) are recommended as most useful for general purposes.

To gain more exact ideas of the principles which must guide one in the employment of hydrotherapeutics, and of the various methods to be used, we will give short *résumés* of the directions for the treatment of some of those diseases in which hydropathy claims to have been successful. To commence with gout, we find that in the acute disease a local antiphlogistic practice is recommended, viz. cold immersion to the part affected, cold



compresses often renewed, or cold or cool compresses covered with a non-conducting body, such as wool, and occasionally removed for a time. But in the chronic forms the local manifestations are disregarded, and treatment is to be directed entirely to the patient. With depression of strength, pale skin, spare muscles, so long as there is no serious complication on the part of the brain, lungs, or heart, general cold douches, of short duration, or friction with moist but tightly-wrung cloths are advised.

If the patient has a dry skin, and is strong enough to bear prolonged heat, the hot-air or vapour bath may be employed, so as to produce sweating, and this may be followed by a cold or cool application. The general douche may be preceded, in certain cases, by a local douche to such joints as are engorged, or are stiff from saline deposit.

Cases with great nervous excitability are best treated by the use of the wet pack or half pack, lasting an hour and a half, and immediately followed by an immersion or friction with a damp cloth.

Supposing the heart, lungs, or brain to be affected, hydro-pathy should be employed with caution, a douche of short duration, of slight force at the upper part, but stronger at the lower part of the body, being advisable; and complicating affections of the stomach or intestine should be treated locally at the same time as the general condition. In the case of plethoric patients the douche should not be so forcible and cold as to provoke violent reaction, and in all cases a restricted diet, abstinence from alcoholic drinks, and, if possible, regular exercise should be enforced.

The objects of the treatment are summed up as follows :

“To favour the operations of assimilation and metamorphosis (désassimilation), to facilitate the secretions, and especially those of the skin and kidneys, to regulate innervation, to prevent visceral congestions by encouraging the cutaneous circulation, and, finally, to restore equilibrium to all the functions of the body.”

Nearly the same principles seem to be held in view in the treatment of “gravel;” stimulate the functions of the organism by general cold douches, excite the skin by the use of heat and of cold water, and keep up the action of the kidneys by the free ingestion of water; and, under the head of diabetes, directions not very dissimilar are given, namely, general cold douches when the functions of the body seem exhausted; hot douche or hot-air bath if the skin be dry, perspiration scanty, and the circulation inactive, avoiding at the same time all applications

to the surface, which may induce the furuncles and local inflammations observed in this disease.

It is recommended in obesity for its powerful action on the capillary circulation on the cutaneous perspiration, on the secretion, and on the majority of the functions, which are so much changed in this condition, though it is clearly recognised that dieting, regular exercise, inhalations of oxygen, repeated purgatives, alkaline and purgative mineral waters are sufficient to effect a cure. The methods to be employed are, first, the exclusive use of forced sweating; secondly, the cold douche, preceded from time to time by perspiration or simple warmth.

It is, however, especially in diseases of the joints and affections of the nervous system that the results of hydropathy are said to be most satisfactory, and we will therefore shortly point out the principal means of its application in the complaints forming these groups.

Articular diseases, excluding rheumatism, are treated under six heads, namely, sprain, chronic hydrarthrosis, arthritis, white swelling, dry arthritis or morbus coxæ senilis, and anchylosis.

For the first, we are not surprised to see that cold water is recommended to relieve pain and swelling, and to prevent consecutive inflammation; it may be employed in the form of local baths, compresses, or irrigation. To combat the puffiness of the joint and weakness of the ligaments that result, excitant applications, especially the local cold douche, must be employed.

For chronic hydrarthrosis the author uses a general cold douche, preceded by a localised douche; and if the patient has rheumatic or scrofulous tendencies the action of the skin is stimulated by packings or hot-air baths, followed by the application of cold excitant applications.

The rules for the treatment of arthritis are not very different: in the acute stage, cool compresses, water baths, continuous irrigation; in the chronic stage, heat and cold applied generally to renovate the organism and favour the processes of assimilation and metamorphosis, as well as local stimulants in the form of steam douches, cold douches, and alternating douches. White swelling is similarly treated; and for the relief of dry arthritis we are told to calm the first symptoms by a Scotch douche or by sweating, followed by a douche or cold friction. If, however, the disease has already produced deformity, general diaphoresis and cold applications must be used to favour organic changes, stimulate the functions of the skin, and improve the general strength, while stimulants (local douche, wet frictions) must be employed to the joint to increase nutrition in the surrounding tissues.

In the operations of straightening partially anchylosed articu-



lations and submitting stiff joints to passive movement, local douches will allay pain and help to prevent subsequent inflammatory processes.

The chapters on diseases of the nervous system are five in number, occupy 250 pages of the work, and treat in order of neuroses, neuralgias, convulsive affections, paralytic affections, and organic diseases of the brain, spinal cord, and nerves, with mental alienation.

Here, as before, symptoms of excitement are combated by the sedative action of prolonged cold; and those of exhaustion or depression, by short stimulant applications, preceded or not by warmth. The development of the nervous diathesis, nervous cachexia (*état nerveux*), is divided into three periods, characterised respectively by the excitation, by the perversion, and by the exhaustion of the nervous forces.

Sedative applications must be used in the first case and stimulant in the last; but in the second, "which is nothing but a condition intermediate between excitement and exhaustion, it will be necessary to combine, in due proportions, the sedative and stimulant agents."

Vertigo may be treated by stimulants, such as a shower-bath or douche; sleeplessness by the sedative action of repeated affusions, cool baths, the wet pack or half pack; and functional locomotor ataxia by diminishing the excitability of the brain and spinal cord. Hysteria, catalepsy, epilepsy, are treated on similar principles, pain and spasm being considered as evidence of excitement requiring sedative applications, and paralysis as a condition to be met by stimulants; for the treatment of the general condition, as contrasted with the fit, the diathesis or constitution of the patient serves as a guide in the selection of the method to be employed.

Dr. Beni-Barde speaks with confidence of the great value of the hydropathic treatment in neuralgia, considering its action more prompt and salutary than the usual agents, subcutaneous injections, vesication, and frictions of various kinds. He prefers to combine cold with heat, and especially recommends the douche écossaise applied to the painful part. The hot-air bath may be also used, and gives good results, but caution is required in the selection of cases, as in some patients it may lead to excessive fatigue or syncope; the temperature employed should be from 40° C. to 70° C. (104° F. to 158° F.), and a cold application should follow upon it. Vapour baths, fumigations, Russian and Turkish baths, and cold applications, have been serviceable occasionally, and in all cases the general condition or diathesis of the patient requires to be carefully considered.

In spinal irritation, the treatment of the general condition is

looked upon as the first step necessary, the special indications being fulfilled by the use of cold douches to the posterior part of the body.

Under the third head, viz. convulsive affections, are discussed painful and painless tics, writers' and other cramps, contractions of the extremities, tetanus, tremors, and paralysis agitans. The author claims to have successfully treated cases of tic douloureux, and recommends for this the hot-air bath (*étuve à la lampe*), followed by a cold application, or the cold douche alone; but in this and other affections, *e.g.* painless tics and professional cramps, the few cases recorded are perhaps scarcely convincing, the duration of the treatment being long, and other circumstances, especially rest, seeming to contribute to the fortunate result.

For all such forms of paralysis as result from cachexia, anæmia, and functional disorder, including the case of reflex paraplegia, the tonic or renovating method of treatment is applicable, but little more can be done than attempt to relieve symptoms, in the case of organic diseases of the cerebral nervous system.

Such is a review of some of the means of curing disease by hydropathy as now practised. Complicated as the machinery of the art appears to be, its principles are almost too simple; for if the local applications of cold and heat in general use be excluded, the choice of the treatment seems to depend entirely on the general condition of the patient, whether it be one of anæmia and depression, requiring stimulation; or one of excitement and irritability, to be met by sedative agents. It is much to be doubted whether the distinction between these two states is properly drawn by hydropathists, especially in the case of nervous diseases, where pain, spasm, and muscular movements are all regarded as evidence of excitability demanding a depressing treatment. There is, however, a large class of complaints which, if attacked unscientifically by the water doctor, receive no less empirical treatment at the hands of the physician; the quinine of the latter being equivalent to the douche of the former; and the cases in the two departments are further assimilated by the aid which can be afforded to both by change of scene, freedom from care, moderation in diet, and regular exercise.

What hydropathists have confessed their inability to relieve, general physicians have been investigating in a more scientific spirit and with considerable success. The uses of diaphoresis in Bright's disease, of the continuous bath in cutaneous affections, and of cold water in fever, do not receive the approbation of our author, though he has occasionally seen good results,



and consequently but very little space is devoted to their consideration.

On the last of these subjects a few words will not be amiss, looking at the impulse the practice has received the last few years.

A reference to our standard text-books on medicine will show that, in spite of the advocacy of Dr. Armitage in 1852, the cold-water treatment of fever has gained but little ground in England. Most authors, indeed, recommend the use of cold applications to the head in the cerebral complications of continued fever, and occasional sponging of the surface of the body, either for the purpose of abating excessive heat, or as a part of the hygienic treatment; and in the article on Scarlet Fever in Dr. Russell Reynolds' 'System of Medicine,' Dr. Gee advises the use of cold affusion and the wet pack in malignant forms of the disease.

But neither the writers of the articles on Enteric Fever and on Typhus in the same work, nor Sir Thomas Watson in the recently published edition of his Lectures, make any mention of the frequent and systematic use of the bath, as a means of reducing the fever and guiding the disease to a successful termination. It is to this method of treatment, as largely employed of late years on the Continent, that we wish to draw attention in the succeeding pages, and we are indebted for much of our information on the subject to papers published in German journals, and especially in the 'Deutsches Archiv für Klinische Medecin,' which, however, refer nearly exclusively to the treatment of typhoid fever. The principle of the treatment may be briefly enunciated as follows: A greatly increased temperature acting for a long time is proved to have a most deleterious effect upon the circulatory, respiratory, and nervous systems, and probably induces even structural changes in many of the organs of the body. The objects of treatment should be to prevent the temperature rising above a certain level, or to maintain the mean height of the temperature as many degrees as possible below that which characterises fevers not treated by cooling agents.

In a series of articles in the first volume of the 'Deutsches Archiv,' Dr. Liebermeister, of Basle, discusses the nature and cause of malignity in diseases, and after showing that affections of the most different kinds—both infectious, as the exanthemata, and non-infectious, as pneumonia and acute rheumatism—may assume the malignant type, he comes to the conclusion that the only commonly recurring peculiarity of these cases is the high degree of fever as measured by the thermometer.

That increased heat of the body is sufficient in itself to pro-

duce serious symptoms is shown by the experiment of Bartels, in which a healthy man was subjected to a high temperature, and suffered successively sensation of heat, with discomfort, anxiety, and feeling of oppression; disquietude; great discomfort, exhaustion, and weakness, uncertain movements; increased frequency of the beats of the heart; sighing respiration; headache, stupefaction, and sensation of commencing giddiness; and finally the experiment was stopped on account of fainting, the temperature being at  $42^{\circ}\text{C}$ . ( $107.6^{\circ}\text{F}$ .), and the pulse 172.

Of more importance still are the results of examinations which Liebermeister conducted on the viscera of patients dying with elevated temperature. The hepatic cells were found either opaque, with finely-granular masses, with or without nucleus, or entirely broken up; opacity, and fatty degeneration of secreting cells, or their destruction, was observed in the cortical substance of the kidneys; and in the muscular tissue of the heart, commencing fatty degeneration. He shows further that in fevers the frequency of the pulse is on an average proportional to the increase of temperature, and that the disturbances, muscular, sensory, and mental, referable to the nervous system, become more serious as the heat of the body is raised. Typhoid fever, malignant malarious diseases, erysipelas, articular rheumatism, and pneumonia, are instanced as affections both infectious and non-infectious, unlike in their symptoms and origin, but agreeing in their liability to produce a morbid elevation of temperature, accompanied by severe nervous symptoms.

If this fatal influence of heat be admitted, then a justification is afforded for the attempt to reduce it within limits, whether by the internal use of quinine and calomel, or by the external use of cold water. But the cooling treatment of fever hardly receives so much support from the foregoing considerations as it does from the test of experiment and practice to which it has been put, as recorded in several papers, and among them those to which we have referred.

The details of the treatment have varied a little in the hands of different physicians, but the following sufficiently well indicates the usual practice pursued:—Thermometric observations are made day and night every two or every three hours, and whenever the temperature is noticed to have reached  $39.5^{\circ}\text{C}$ . ( $103.1^{\circ}\text{F}$ .), the patient receives a bath at  $16^{\circ}\text{R}$ ., to  $20^{\circ}\text{R}$ . ( $58^{\circ}\text{F}$ . to  $77^{\circ}\text{F}$ .), in which he remains ten or fifteen minutes, the surface of the body being gently rubbed by the hands of assistants to ensure complete contact of the skin with the water. He is then dried, and placed again in bed. If at the next observation the thermometer again records  $39.5^{\circ}\text{C}$ ., the bath is repeated, so that in some severe cases under Liebermeister the



operation has been gone through 150, 160, or even 180 times in the course of the fever.

The differences referred to above have concerned chiefly the limiting of temperature, which Liebermeister placed at  $40^{\circ}$  C., and Leube at  $39^{\circ}$  C., and the duration of the bath, which was sometimes determined by the occurrence of shivering, and sometimes was continued until the temperature had fallen to  $37.5^{\circ}$  C. ( $99.5^{\circ}$  F.).

Böhm and Michel also applied, at temperatures between  $101^{\circ}$  F. and  $103^{\circ}$  F., moist compresses to the shoulders, chest, and belly, and an ice-bladder to the head.

In reviewing the results of this treatment in enteric fever, we shall first inquire in what way the mortality of the disease was influenced. The great variability in the severity of the disease, and the liability to confound the slighter degrees with mere gastric disturbance and pyrexia, constitute a difficulty which Liebermeister<sup>1</sup> has taken into full consideration. He states the mortality at Basle, in the years 1843 to 1864, was 27.3 per cent.; but that in the same town, from August 22nd, 1865, to Dec. 31st, 1867, when antipyretic methods of treatment were in use, of 1178 cases only 163 died, representing a percentage of 13.8. Of these many were slight cases of fever with abdominal catarrh, on the exclusion of which the number of cases would be reduced to 924, the fatal cases remaining at 163, and the percentage reaching 17.6—a number considerably below that representing the death rate of former years. On the other hand, by adding to the typhoid cases of earlier years the milder gastric fevers, the mortality, as compared with that in 1866 and 1867, would still be much greater, namely, as 18 per cent. to 13.8 per cent. During the war of 1870, Böhm and Michel treated 131 cases of typhoid in a lazareth opened in the Curhaus of Niederbronn, for the purpose of receiving sick and wounded from the besieging corps at Bitsch and from the troops in the neighbourhood; we may therefore suppose that a large proportion of cases would be of a dangerous type, and, indeed, 53 are mentioned as being severe. Of the whole number, 15 died, forming a percentage of 11.45. The spleen was enlarged in all the cases which came under treatment, and intestinal symptoms were present in almost all; of these, 9 patients suffered from hæmorrhage, which was fatal twice; and ulceration of the larynx was observed in 30 patients, of whom 7 died; croupous pneumonia also occurred once, and was fatal.

Of 125 patients treated by Dr. Scholz, 5 only (4 per cent.) succumbed to the disease; 2 of these deaths are ascribed to a

late admission and insufficient use of the baths, the mean temperature being allowed to remain too high; a third was admitted even later (the 17th day), and a fourth died from abundant intestinal hemorrhage.

In 1870, F. Riegel treated 87 patients, with 4 deaths; and in 1871, 69 cases with 3 deaths, the percentage of the whole number being 4·4.

Two late admissions, two cases of pyæmia from gangrene of the leg and bed sore, respectively, and one case of perforation with suppurative peritonitis, with two others, constitute the death list.

The mortality among the patients treated by Götz was higher, namely, 15·4 per cent., or 13 out of 86; but of these only 54 were bathed, with 3 deaths; and among 31 in whom this method was not pursued on account of low temperatures, collapse, or hæmorrhage, so many as 10 fell victims.

Merkel, in 1869, only lost 2 out of 41, or 4·5 per cent.; but Leube, in the winter of 1870-71, did not succeed in getting a lower mortality than 18·5, or 13 in 70.

From a consideration of the total number of cases mentioned, it appears that out of 1533 cases treated by different physicians on the method under review, 218 were fatal, giving a percentage of 14·22; though with one series the mortality was only 4, with another 18·5.

The whole result appears more favorable than that recorded by Dr. Murchison in the new edition of his work on 'Continued Fever;' according to which the mortality at the London Fever Hospital, between 1848 and 1859, was 17·26, and that of a great number of cases occurring in Great Britain and foreign countries scarcely differed, being 17·45. But it seems quite possible that variations in the character of the epidemics, in the dietetic and hygienic arrangements, and especially differences in the principles of diagnosis of the milder cases, may account for some of the diminution in mortality, which is observable in the cases we have analysed.

If we examine, on the other hand, the observations of those who have compared the bath treatment with that used in the same hospital in former years, or even in other departments of the hospital in the same year, we shall see that the mortality is very decidedly less. Thus at Basle the percentage under the old treatment was 27·3, under the new (Liebermeister) 17·6; at Würzburg, more than 20 per cent. died under old methods, and only 4·4 in the hands of Riegel; and with Götz, these mortalities were respectively 28·72 and 15·4. Merkel also speaks of the marked diminution of mortality.

We will now refer to the effects of the cold-water applications



on the special symptoms of the disease, and on the development and progress of the numerous complications which may arise.

Their beneficial results as compared with those of other hitherto used methods, are thus described by Liebermeister, speaking from the experience of more than a thousand cases:

“A decided typhoid condition was exceedingly rare except in those in whom it already existed on admission. The frequency and intensity of the mental disturbances were still more diminished by the cold-water treatment, than had been the case with less energetic antipyretic means.

“Dryness of the tongue, sordes on the tongue, lips, and gums, were extremely rare in those who came early under treatment; and in many patients, even when these symptoms were present on admission, they improved soon after the commencement of the treatment.

“With the cold-water method, the frequency of the pulse was not so high as in cases treated otherwise, and especially the greatest degrees of frequency corresponding to commencing paralysis of the heart, were very rare: the rapidity of the pulse very often diminished soon after the beginning of the treatment.

“The intestinal affection also seemed to be favorably influenced by the cold-water treatment; the motions were on an average less in number; higher degrees of meteorismus were very rare, and even intestinal hæmorrhages and perforations occurred less frequently. On account of the improved condition of the mental functions, the discharge of urine and fæces was seldom performed unconsciously.

“Serious lung affections were less often observed with the treatment under consideration than with other methods; if they do occur, they do not in general contraindicate the continuation of the cooling system. Bronchial catarrh also was on an average less violent.

“Extensive bedsores occurred only once among those patients who came early under treatment, and in all cases the complication was much rarer than in those treated without frequent abstraction of heat.”

Scholz is explicit as to the influence of the treatment on the course of typhoid fever; it had, he says, no influence on the typical course of the disease, determined by the successive changes in the intestinal mucous membranes, the ulcers and patches being always found in a state corresponding to the duration of the case. But the alleviation of symptoms was sufficiently marked: firstly, the effect on the nervous system was most striking; mental confusion and delirium scarcely occurred, or yielded to the energetic use of baths; the pulse rarely exceeded 100, and bronchitis was slight in those coming early under treatment; pneumonia was present in only seven

cases, or 4·1 per cent., whereas it affected 20 per cent. of those treated otherwise; the stools were less numerous, and only five times (or 3·8 per cent.) did hæmorrhage take place from the intestine; the skin was kept moist, cool, and pleasant, and bed-sores formed in only five patients.

From the results of his treatment of forty-one cases on this system, Merkel was disposed to think that the duration was somewhat lengthened, and not giving so favorable a report of it as had up to that time appeared, only claimed for it a lessening of the mortality and an alleviation of the symptoms. Delirium and involuntary evacuations generally ceased soon after the use of baths; abundant diarrhœa was noticed only in three cases; intestinal hæmorrhage in two; albuminuria occurring in some cases, was observed almost always after the use of the bath. This symptom was very frequent in Leube's cases, but was not attributed by him to the cold-water applications; bedsores were entirely absent, but this might have been due to the use of water and air pillows: the diarrhœa was in all cases marked, the bronchitis moderate, and the eruption sparing.

The contra-indications which are chiefly mentioned by those who have employed the cold-water method, are hæmorrhage from the bowels, severe collapse, and high degrees of weakness of the heart. Neither bronchitis, pleurisy, nor pneumonia need prevent the employment of refrigerating measures, if the temperature demands them; they have likewise been used without harm during menstruation, and in pregnancy at the fourth month.

It is another question, to what unpleasant consequences the application of cold may itself give rise. It appears to be the general opinion that intestinal hæmorrhage was more frequent, and attributable to the sudden cooling of the external surface. Frequent cases of relapse are noticed both by Merkel and Riegel, and the latter refers to a burning pain in the soles of the feet, which was suffered by nearly half his patients, and was also noticed by Jürgensen and Hagenbach.

It might be supposed that patients, at least when conscious, would object to the discomfort of being placed in a cold bath, and that this would form a serious obstacle to its free employment; but it was pretty generally observed that if the patients complained at first, they willingly yielded after the administration of four or five immersions had shown them that the relief afforded was considerable. An objection, which scarcely exists in the case of hospital patients, has great force when an attempt is made to carry out the treatment in private practice; we refer to the necessity of frequent thermometric observations, and of able and skilful attendants to conduct the operations of immersion. This is one among the reasons which lead Dr. Riegel to recom-



mend, instead of the bath, the continuous application of ice-bags upon the chest and abdomen; he has proved that the refrigerating effect of these may be as great, or even greater, than that of the bath applied in the manner indicated in the foregoing pages, and he claims for it that it obviates, first, the sudden and frequent rises and falls of the temperature, the latter of which may lead to hæmorrhage; secondly, the necessity of so frequent thermometric observations; and he states further, that the body may by their means be kept at a lower mean temperature than by the usual bath process. He would use ice-bags large enough to cover completely the chest and abdomen, and fitting accurately to the surface.

The papers we have consulted refer almost exclusively to the treatment of abdominal typhus or typhoid fever; but Götz gives some of the results of the application of this method to exanthematic typhus. His report is, however, not favorable to the bath, for while in the other divisions, where the usual treatment was pursued, the death rate was 16 in 89, or 17·97 per cent., the mortality of those under his care was 26·16, or 28 in 107. The patients who received baths were 80 in number, those not bathed were 27; of the former 17 died, or 21·25 per cent.; of the latter 11, or 40 per cent.

It is not too much to hope, that the foregoing remarks on the external use of water in acute and chronic diseases, and the notice of the results of experience by physicians in whom every confidence may be placed for scientific accuracy, may induce some of our hospital physicians to seriously and systematically investigate the subject, and test the modes of practice that have been recommended. That the therapeutic use of water has been so largely associated with quackery and knavery, affords no apology to our physicians for neglecting to investigate it, and to place it on a secure and scientific basis.

## VI.—Demarquay on *Regeneration of Organs and Tissues*.<sup>1</sup>

M. DEMARQUAY states in his preface that the task he set himself originally was to make a close study of the mode of regeneration of tendons, but the circle of his experiments soon widened, and he found himself insensibly led on to follow out the changes that take place in a great number of other tissues.

<sup>1</sup> *De la Régénération des Organes et des Tissus*. Par J. N. DEMARQUAY, Chirurgien de la Maison Municipale de Santé, &c. &c., avec quatre planches. Paris, 1874, pp. 328, large 8vo.

The result is the work before us, which really constitutes a very valuable contribution to medical literature.

M. Demarquay commences with an historical *résumé* of the results obtained by previous experimenters in regard to the regeneration of large portions of the body in the lower animals, as in polypes, infusoria, acalephæ, vermes, mollusca, crustacea, insecta, and in the different classes of the vertebrata. He then speaks of the regeneration of certain parts in the higher animals after removal, as the feathers in birds, teeth, nails, hairs, &c., and of the eye, lens, spleen, and kidneys. The proper subject of the treatise commences with the fifth chapter, which is devoted to the process of regeneration in the tissues, which he points out may take place either without inflammation, as in the case of epithelium and cartilage, or may be accompanied by a certain amount of inflammation, as in wounds generally.

Our knowledge of the mode of regeneration of epithelium is not at present quite satisfactory. No evidence has hitherto been obtained of a process of multiplication of the cells of the deeper layers, neither the nuclei nor the cells having been seen in process of division. Thiersch maintained that after removal of the epithelium new epithelium was produced from that surrounding the part injured; and this appears to be the case when the cornea has been abraded. It is certain, however, that islets of epidermis appear often in the middle of an ulcer of the skin, and these may possibly be derived, as Küss supposed, from the cytoblasts remaining on the surface.

In regard to cartilage, M. Demarquay adduces many facts in favour of its regeneration. Those of incrustation, those of the nose, ears, &c., are reproduced with great facility; and he agrees in the conclusions arrived at by Peyraud, that its regeneration is more active in proportion to the abundance of its nutrition; that the new cartilage resembles in appearance foetal cartilage, and does not ossify for some time. The tissue first formed, however, appears to be of a fibrous nature, in which the cartilaginous elements subsequently form. The new tissue seems to come from the old; or, if the section have exposed a large surface, from the neighbouring perichondrium.

In regard to muscular tissue, whilst admitting that most authors and experimenters are opposed to the idea of its reproduction after section, M. Demarquay gives the details of a series of microscopical researches he has made which lead him decidedly to give the weight of his testimony to those who are of opinion that under favorable circumstances it can be reproduced.

In regard to the nerves and nervous centres, he only quotes the experiments of others, and it is unnecessary, therefore, to



say more than he holds with Waller, Schiff, and others, that after section that extremity of the nerve only which is in connection with atrophic centre does not undergo any alteration.

In regard to the regeneration of bone, M. Demarquay agrees generally with the views of Ranvier, and therefore holds that osseous tissue forms in cartilage, and in fibrous tissue almost exactly in the same manner. There is a first or primary stage, in which the fundamental substance of the fibrous and cartilaginous tissues breaks down, the cells become free, proliferate, and thus form the cells of the embryonic marrow. Its second phase is characterised by the transformation of the medullary cells into stellate osteoplasts, and by the formation of a new intercellular substance, the osseous substance itself. To sum up, whether the bone takes origin from cartilage or from fibrous tissue, embryonal elements (embryonal marrow) are first produced, and these elements form osseous corpuscles, and around these corpuscles a new substance appears, in which the proteic elements and the calcareous salts are intimately united. Osseous tissue is formed at the expense of fibrous and cartilaginous tissue by a process of physiological heteroplasia, that is to say, the cellular elements of bone, whether arising from cartilage or fibrous tissue, have a form and function different from those of the formative elements. The osseous substance, in fact, is a new product without analogue in the rest of the organism. In the act of regeneration, the osteoid layer beneath the periosteum, which is formed by the elements of the embryoidal medulla, plays an important part, the cell-elements appearing to return to their embryonal state.

We come now to the consideration of the regeneration of tendon to which M. Demarquay has chiefly devoted himself, and which is here preceded by an historical *aperçu* that shows the author has paid considerable attention to the writings of previous workers; and from the cases he has culled from their works proves to demonstration the value of the suture of the various tendons in cuts about the wrists and other tendinous regions. He gives the details of upwards of fifty experiments he has himself made upon the tendo Achillis and the tendon of the patella. The animals were always carefully fed after the operation, and pains were taken to avoid the extravasation of blood. The phenomena observed were in accordance with the statements made by Held, Duval, Bouvier, Guérin, Jobert, and previously published by himself in 1862, and were essentially as follows: After the lapse of a certain time the tendinous sheath became vascular, the flux to the parts caused a little serous infiltration, and sometimes a little infiltration of blood

into the sheath itself and the adjoining tissue; very shortly the sheath thickened, especially at its extremities, and by degrees the flattening which takes place at the moment of section disappears, and the sheath becomes filled out, especially if there have been any sanguineous oozing into its interior. The work of regeneration commences between the fifth and tenth days, and the elements about to take part in the production of new tendon acquire consistence. The resorption of the serosity and blood around the tendon takes place with rapidity, and, speaking broadly, union is effected in from ten to twenty days. In following with the microscope from day to day in a series of rabbits the changes that occurred, the phenomena observed were, that *two days* after the section the internal surface of the sheath presented a soft gelatinous substance infiltrated with serum, and so adherent to the sheath that it seemed to form a part of it. On examining a portion of this, removed with curved scissors, fasciculi of connective-tissue vessels and elastic fibres came into view, showing that the whole was pre-existing, since they could not be supposed to have been formed in so short a time after the injury. A number of very transparent spherical cellules also appeared, having a diameter of from  $\frac{1}{8000}$  to  $\frac{1}{10000}$  of a millimètre ( $\frac{1}{3125}$  of an inch). These contained one or two ovoid nuclei. They swelled up water, and dissolved easily in acetic acid. Some of them contained a few fat granules round the nucleus, even at this early date, and the granules became more abundant on the following day. These leucocytes he believes probably proceed from the vessels in the way Cohnheim has described, though he does not think the other methods of their possible origin, as from the proliferation of the cells of the tissues, or from free development in blastema, have been altogether controverted. Besides these elements the serosity bathing the clot, and on the surface of the gelatinous substance presents some filamentous sinuous flocculi, which he thinks are mucin. In longitudinal vertical sections of the sheath the outer portions seem not to have undergone any appreciable change. The connective tissue corpuscles appear to be fusiform and elongated parallel to the axis of the tendon. Towards the internal surface of the sheath the cells swell, the nucleus increases in size, divides, and the product of the division being at length set free by the rupture of the parent cell. In the following days, by a repetition of this process, the nuclei become very numerous, and many of them are enclosed in a layer of angular and branched protoplasm. These masses becoming smaller, take the form of stellate cells, and form anastomoses with a small quantity of intermediate substance, and vessels appear in the new tissue



thus constituted. At a later period the cellular elements become more filiform, and take a direction generally parallel to that of the tendon, and the intercellular substance becomes striated longitudinally, and soon becomes capable of being divided into longitudinal fibrils. It then forms the cicatrix.

Thus it appears that it is really the sheath which serves to form the tendinous cicatrix, and which furnishes the material for what is termed tendinous regeneration. It is its own proper substance that makes the first efforts at this restoration; but soon the pre-existing elements become only accessory, surrounded and lost as they are in elements of new formation proceeding from the proliferation of the connective-tissue corpuscles; and it is these new elements that subsequently become stellate corpuscles, and form the web of fibrous tissue interposed between the two extremities of the divided tendon. And now, in regard to these extremities, they begin to swell about the fourth day and gradually increase in size. At about this period, whilst still presenting the abrupt termination caused by the section, they begin to contract adhesions to the gelatinous substance, which at this time is tolerably firm and forms a complete investment for the clot. By degrees the adhesion of the tendon with the interposed tissue proceeding from the periphery towards the centre becomes complete. There is no longer mere contiguity, but direct continuity between the two tissues, the tendinous tissue and the tissue of the cicatrix.

Histological examination demonstrates that up to the third day little change is observable in the tendon. At this time, however, the thin and elongated fusiform corpuscles of the tendon swell up and their nuclei become more distinct. At the parts where it is in contact with the sheath a multiplication of the nuclei in the interior of the cells can be seen. These becoming free, arrange themselves longitudinally between the fibres of the tendons, and are the cause of the tumefaction of the free extremities, the fibres of the tendons themselves not appearing to be materially altered. The new elements which never become very much elongated are also the means by which the connective tissue of the tendons fuses with the interposed tissue and the new material thrown out by the sheath, the union always proceeding from the periphery towards the centre, and soon the fusion is so intimate that it is impossible to separate the parts by traction. The presence of these linear collections of cellules long remain perceptible, even to the naked eye; the tendinous fibres between them lose their nacreous aspect and become striated, yellowish, and slightly granular bands.

Lastly, in regard to the clot. Surrounded by the gelatinous

substance, which is about to constitute the cicatrix, it loses its serum by absorption, as well as a part of the hæmatosine. The globules remain with a part of the hæmatosine, and pass through the usual changes, by which it becomes converted into a pulpy mass, rich in proteic and fatty granules, and in granules or sometimes in rhombohedric crystals of hæmatoidin. Gradually the more soluble parts disappear, and at length only an ochraceous spot remains, which may itself ultimately disappear, the particles being perhaps removed by the agency of the lymphatic system.

The succeeding chapters of M. Demarquay's work deal with the subject of the suture of tendons and of nerves, affording many examples of the advantage that may be derived from this proceeding, not only when the injury is recent, but after it has long been recovered from. The last chapter is occupied with a consideration of the principal conditions which favour or hinder regeneration. These he divides into two groups—those which are due to the subject, and those dependent on external circumstances. Amongst the former he enumerates the influence of nutrition, of the pathological state, of age, of sex, type of the animal, race, and species, of the circulation, and of the nervous system. Amongst the latter he includes the influence of light, temperature, and air; and last, the influence of irritation and inflammation.

In conclusion, we may observe that M. Demarquay's work is well illustrated by four plates, in colours, containing a good many figures, and representing the successive changes, both macroscopical and microscopical, that take place after division of the tendons, both in the tendons themselves and in the surrounding parts. The work, as a whole, is well worthy of perusal, and, as we have pointed out, not only contains an excellent historical *résumé* of the subject of "regeneration" in the various tissues, but the results of much careful experimentation on the part of the author himself.

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## VII.—Collected Works of Cæsar Hawkins.<sup>1</sup>

THE perusal of Mr. Cæsar Hawkins' two volumes gives one the same kind of pleasure as is produced by revisiting some once familiar neighbourhood, and renewing our acquaintance with its many well-known, but to some extent forgotten,

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<sup>1</sup> *Contributions to Pathology and Surgery.* By CÆSAR H. HAWKINS, F.R.S., &c. London, 1874.



features. Here are the old landmarks to which our memories have often involuntarily referred; here the types which have so often furnished us with standards of comparison; here the characteristic scenery which has so quietly though unconsciously tintured our imagination of quite other scenes. Here, too, is the hill which we with difficulty climbed, and whence there was revealed to us a surrounding country of which we previously had no idea; and we are surprised to find how much of these almost forgotten first impressions enters into our present ideas and influences our existing opinions.

Thus, Mr. Hawkins' essays are something more than historical monuments of many important steps in the progress of surgery and pathology, and apart from the interest which necessarily pertains to them as such, we find amongst them some of the prominent elements of which the edifice of our existing surgical knowledge is built up. We are glad that Mr. Hawkins has not attempted any process of alteration or "restoration" (we believe that is the term usually applied to the defacing of ancient work with a view to bringing it into impossible harmony with the present time), for this would have destroyed much of the interest of the books; and we are, moreover, doubtful whether even the master's own touches are often likely to improve work originally good, which he has produced at any considerable interval of time.

Mr. Hawkins has contented himself, wisely we think, with reprinting and collecting into these volumes his numerous lectures and essays which have hitherto been only with difficulty accessible, owing to their being scattered through a long series of periodical publications.

These contributions to surgery and pathology extend over a quarter of a century, and embrace a great variety of subjects, many of them having originally been delivered as clinical lectures at St. George's Hospital, to which institution their author is still one of the consulting surgeons.

One notable character of these essays is that judicial tone and impartial sagacity which have made Mr. Hawkins' judgment on difficult questions of so great value; and they are written with a clearness of diction that shows a thorough mastery of the subject treated.

The volumes open with the Hunterian oration delivered at the College of Surgeons in 1849. This is an eloquent tribute to the noble qualities upon which Hunter's greatness depended, and could only have been written by one whose cultivation and accomplishments rendered him able to appreciate in an unusual degree the character of the remarkable man whom he commemorated, and to whom, Mr. Hawkins says, "no one will

refuse a place among those bright luminaries of science from whom, as from the more distant heavenly bodies, light may still continue to reach us, even for centuries after they have themselves become extinct." It is well shown, too, how "vast and varied a fund of original knowledge was actually possessed by Hunter, in many points unsuspected, till brought to light by kindred minds, and explained by the advance of science since his time;" and that there are to be found thickly scattered through his writings many half-formed theories and hypotheses "which have been proved by further investigation, with increased collateral knowledge, to have been most sagacious anticipations of important facts." Indeed, the reperusal of this oration reminds us how many of the so-called novelties of the present day are only old ideas renewed, and adds to our conviction of the truth of Mr. Hawkins' statement that "it is a common error to mistake fresh terms for new ideas, and merely altered explanations for real acquisition of knowledge."

The lecture on hydrophobia contains what is still one of the most graphic descriptions of this frightful disease, as well as Mr. Youatt's account of the symptoms of rabies in the dog; it may be read with much advantage as an antidote to the numerous errors still prevailing upon the subject. Especially useful may it be to note the facts that the disease is invariably produced by inoculation, and that the rabid dog, so far from being known by his horror of water, is affected by an intense thirst, which it takes every opportunity of attempting to assuage. It may help to prevent needless alarm also to know that Mr. Youatt kept a record of four hundred persons who had recourse to his assistance after having been bitten by really rabid animals, and although one died of fright, not one had hydrophobia, which, as Mr. Hawkins says, if confirmed by other documents, leaves little ground for apprehension to any one who has applied for surgical assistance. Doubtless many a dog has been condemned as mad who was merely suffering from exposure to heat and a sort of temporary delirium, for which a pail of cold water would have been found an effectual and speedy remedy.

A very interesting paper is that on the diagnosis of foreign bodies in the larynx, which, though dated 1840, might have been written to-day. It clearly points out how many circumstances may modify the symptoms which are usually considered characteristic, and shows the necessity of judging each case by its own particular history and peculiarity.

With regard to the stethoscopic signs of the impaction of a foreign body in the bronchus, we may, in passing, mention a case in which the substance (a piece of tobacco pipe), though



lodged in the right bronchus, yet projected across the bifurcation of the trachea in such a manner as to almost entirely occlude the left bronchus also.

Probably one of the best known and most valuable papers in these volumes is that upon colotomy for stricture of the colon, with its accompanying analysis of forty-four cases of artificial anus: a paper to which every subsequent writer on the subject has been much indebted. It is needless to remark on so well-known an essay, but we may note concerning Mr. Hawkins' statement that "there is no known instance of recovery after an artificial anus has been intentionally made in the small intestine, except when there has been a protrusion of a hernia," that a case has been recorded by Mr. M'Carthy in the 'Medico-Chirurgical Transactions' of 1872, in which the patient lived forty-eight days after an opening had been made in the ileum for an obstruction due to cancer of the splenic flexure of the colon, and apparently died from heart disease.

The paper on "Excision of the Ovarium" is interesting, as recording the first successful case occurring in a London hospital: this was in 1846.

There are some exceedingly practical lectures on diseases of the nose, on epulis, on nævi, and on diseases of bone, which, however, do not call for special remark, but we cannot pass on without noticing the lectures on congenital cystic tumours, which relate to a disease not often described, and not unfrequently mistaken. Mr. Hawkins well shows, both by the relation of cases and by post-mortem dissections, the difficulties that would attend the removal of these tumours, and points out the appropriate treatment.

In the paper on "Concussion" there are some judicious remarks upon bloodletting in injuries of the head, which shows that even in 1832 our author was quite alive to the evils attending the indiscriminate use of this remedy, and some most useful cautions concerning the way in which the symptoms of the disease may be masked by the remedial bleeding.

The following remarks, written in 1841, are interesting when read in the light of the recent discussions on pyæmia. The occurrence of secondary abscesses, Mr. Hawkins says—

"Is undoubtedly much under the influence of atmospheric causes of the same kind exactly as those which occasion erysipelas and inflammation of cellular tissue and serous membranes; so that at one time almost every case is in danger of dying of one of these affections, while you will, at another time, see several months elapse without witnessing a single case."

The lecture on cheloid tumours gives an admirable descrip-

tion of that rare disease, and that on the diagnosis of the various ulcers of the tongue is especially deserving of study.

The lectures on tumours exhibit, of course, a nomenclature differing a good deal from those chiefly in use at the present day, but they contain a vast amount of valuable material, and the descriptions may still serve as very safe clinical guides. With the exception of the microscopical characters, it would be difficult to add anything to the account of "the tubercular conglobate tumour," by which name Mr. Hawkins describes the disease which has since been associated with the name of Dr. Hodgkin. A more accurate discrimination than was attempted by Mr. Hawkins, in these lectures, of the several kinds of tumours of the bones has, however, doubtless, since then, become possible.

In his lecture on hospital gangrene, Mr. Hawkins, while admitting that the disease may be conveyed by contagion, gives ample reasons for believing it to be chiefly due to atmospheric causes :

"The term hospital gangrene is therefore applicable to the disease because it often affects many patients in an hospital, and not because it is dependent upon any peculiar circumstance connected with the hospital itself, except so far as any disease is made worse in an hospital. It is caused, I think, by the condition of the atmosphere at the time, and I have generally noticed it when the state of the air has been such as to cause erysipelas, and other diseases of a low type."

Besides those to which we have alluded, the volumes contain many other interesting clinical lectures, among which we would especially note those on diseases of the testis, on contraction of the palmar fascia, and on aneurism. But we have probably said enough to show how much of interest these volumes contain. The collection of these writings must, we should think, have involved a pleasant retrospect for their accomplished author, to whom the profession owes a fresh debt of gratitude for presenting them in this convenient form.

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### VIII.—Lund's Surgical Experience.

A FEW years ago Mr. Lund published a pamphlet in which he advocated careful note-taking, not merely in the most im-

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<sup>1</sup> *Five Years' Surgical Work in the Manchester Royal Infirmary.* By EDWARD LUND, F.R.C.S., one of the Surgeons to the Hospital, and Member of the Senate of Owen's College, Manchester. Reprinted from the 'Liverpool and Manchester Medical and Surgical Reports, 1874.' Manchester, 1874.



portant and unusual cases, but in all cases alike; and he suggested a plan by which this might be done with brevity and clearness. This pamphlet we noticed at the time. He has since given his plan a trial in the wards of the Manchester Infirmary, and he now lays before the profession a *résumé* of the surgical work that has fallen to his lot in that institution during the last five years.

We congratulate the author upon the evidence which this little volume affords of the value of his system of note-taking. We have here a record of *all* the cases that have passed through his wards; in some instances the record is of the briefest possible kind, but wherever it is in any way necessary, or wherever it is at all interesting, the particulars of the case are given with more or less of detail. Thus, the system advocated is not a mere tabulation of statements; but possesses an elasticity which allows the surgeon to bring out all the important features in his cases. This is very apparent to any one who reads Mr. Lund's small book with attention; for, although some pages are occupied by mere tables, containing very little beyond figures and letters, yet there is a running commentary in which all the most important surgical questions of the day are discussed. For example, antiseptic dressings in all their variety, skin-grafting, the arrest of hæmorrhage by acupressure or by torsion,—these and many other moot points come under consideration.

Perhaps the best course that we can adopt in noticing a work of this kind is to give our readers a few extracts from those parts which are of the most general interest. We cannot copy tables of figures; and, if we could, our readers would not thank us for laying before them fragments of statistics. But the commentaries which hang upon these statistics are often very instructive and interesting.

Among the cases of gunshot wounds the following is both curious and instructive:

“One of the most peculiar was that of a man, aged 32, who, about five months before his admission into the infirmary, had sustained a gunshot wound from the explosion of a double-barelled gun while firing it. Part of the stock of the gun had lacerated the left forearm on its internal side; the wound was two inches long, and was partly healed up in about three weeks, but never completely so, as there always existed at the lower part of it a small sinus or aperture. This remained open until the time of his admission into the infirmary, which was nearly five months from the date of the accident. During this long period it was observed that from time to time he was liable to sudden bleeding from the wound, particularly after slight muscular exertion or any peculiar movements of the limb.

When I saw the case I determined to place him in the hospital, and, if necessary, to cut down upon the track of the wound and remove any portion of bone or foreign matter which might be found there. He was first taken into the infirmary, with the object of merely watching him to see if it was really true that he had these sudden attacks of profuse hæmorrhage, which he said had occurred more than twenty times since the injury, lasting for a while and then ceasing spontaneously. About the fifth day after his admission, when he said it bled for the twenty-ninth time, it suddenly commenced bleeding, a large stream of arterial blood spurting out of the wound. A tourniquet was placed on the arm, and, as I was fortunately in the hospital at the time, chloroform was administered, and I proceeded to carry out my plan of action. Passing a probe along what appeared to be only a sinus, I felt something which seemed to be like a piece of loose dead bone. Still further enlarging the opening, I could feel it plainly, and once more increasing the size of the wound I proceeded to draw it out with a pair of necrosis forceps, when, to my astonishment, it turned out to be, not a portion of bone, but a large piece of the breech of the gun-barrel, nearly three inches in length and three quarters of an inch in width, and forming very nearly one quarter of the circumference of the barrel. This being removed it was found that underneath it there was an opening in the anterior interosseous artery, upon which one end of the piece of iron had rested, its concave surface fitting round the shaft of the ulna, compressing the artery. This seemed to have acted as a valve, and each time the limb was moved in one particular direction the artery was released from pressure and the blood escaped. Then by some other change of place it compressed the vessel, and stopped the bleeding. In this way only can I account for the very unusual condition that the wound should have so nearly healed, and that the man should have experienced such a number of separate attacks of bleeding as I have described. The artery was now divided transversely, the ends tied with catgut ligature, and the wound treated with strips of lint dipped in a solution of tannin; and in less than four weeks from the date of the operation the patient was discharged perfectly cured."

Here is another case of gunshot wound which is worth noticing, if it were only to point out the almost incredible carelessness that prevails in the use of firearms :

"In one case a man who was fatally injured by the explosion of a gun explained to me, on his admission to the hospital, and while quite conscious, that he was out shooting small birds, and in order that the birds might not become alarmed by the sight of the gun, he placed it, muzzle downwards, deeply in a very long pocket in a flap of his coat, so that the trigger even was within the pocket. He walked along with it in this careless manner, and when attempting to pull it out of the pocket the trigger caught in the lining, the gun went off, and the contents were lodged in his leg and foot, from the



effects of which he died in a few hours after his admission and before amputation could be performed."

Mr. Lund has entered into the merits of antiseptic surgery more fully than most surgeons, and, of course, he has used carbolic acid largely. Here is a case which shows that that substance is not altogether free from danger, and that, at least in strong solutions, it ought to be employed with caution:

"One abscess which I had to treat was situated in the cellular tissue around and behind the cæcum, in a woman, aged 43 years. A small aperture, consequent upon the spontaneous bursting of the abscess, had existed for a long time, and gave me considerable trouble. I can hardly say that any material benefit had accrued even after some months' treatment. But in this case I had an illustration of the dangers which may occur from the indiscriminate use of carbolic acid; for, in the treatment, the first thing I did was to enlarge the opening which nature had formed for the exit of matter, and thinking I might possibly arrest the further secretion of it, and diminish its putrid character by the application of strong carbolic acid, I injected, by means of a syringe with a small long nozzle, about a drachm of the nearly pure acid mixed with just enough water to keep it liquid. I had hardly done this when the woman suddenly turned deadly pale, the pulse seemed almost to cease at the wrists, and it appeared as if she was in imminent danger of instant death. This lasted only for a few moments until we could get brandy and other stimulants for her relief, and although she remained in a very exhausted and faint condition for many hours afterwards, fortunately no worse results followed."

The following passage throws light from rather an unexpected quarter upon the pathology of sprains, a class of accidents which are often so tedious and troublesome that we are glad of any information which may enable us to treat them more successfully:

"Some years ago I had an opportunity of seeing the changes in muscular tissue which result from sprains near joints in the lower animals. A wholesale butcher showed me a few carcasses of beasts in which some of the more expensive cuts had to be sacrificed on account of injuries which the animals had received from sudden sprains, whilst being driven along the then rough pavement of the crowded streets of our city. If we consider how such animals as sheep and oxen are accustomed to soft pastures with impressible soil, it will be seen how the unyielding surfaces and stones of our roads must interfere with their ordinary habits of locomotion. Hence it is that in traversing our streets they are apt to suffer sundry sprains and injuries, and so in these cases there had been invariably a distinct laceration of many fibres or bundles of fibres of fleshy tissue near the hip-joints, chiefly in the locality of the principal adductor muscles, as if every now and then in trotting along the animal had suddenly straddled or separated its legs over the

slippery stones, and thus torn the fleshy tissue of certain muscles. In these places I always found an effusion of clotted blood, and was told by the butcher that this quickly putrefied when the beast was cut up, and the meat in these parts soon became so bad that it could not be offered for sale. With this view of what sprains may possibly be we can infer, I think, what are likely to be the best means of treatment. Rest must form one element, and time another, in order to permit of the slow absorption of the effused blood, and the deposition of some new fibrinous matter, which shall act as a connecting medium between the ruptured fibres."

A case of a bursal cyst in the popliteal space, connected with the semimembranosus muscle, in a little girl aged 8, is mentioned at page 50, in which, as it appears to us, the treatment was unnecessarily severe. The cyst was tapped, two drachms of clear fluid were drawn off, and then the same quantity of undiluted compound tincture of iodine was injected. We are not surprised to read that suppuration was set up, with some irritative fever, and that the patient was not well for five weeks. Would not milder measures have effected a cure in a shorter time? Might not the disease have been equally well met by merely painting the surface with tincture of iodine and applying a back-splint with a well-adjusted bandage?

The last case we quote is truly remarkable for the number of calculi that had been formed in the patient's bladder, and for the number of operations to which he had been submitted :

"J. H., aged 61, had been lithotomised a very unusual number of times. In May, 1861, the first calculus had been removed by lateral lithotomy. Ten months later sixteen calculi were extracted by the same operation, and in about the same period afterwards twenty more calculi were taken away. About one year previous to his coming under my observation he had been operated on in the same way by the late Mr. Jordan, who removed four calculi. He recovered from that operation, and then passed spontaneously ten more calculi, of average size, which were voided per urethram. So that in various ways, partly by art and partly by nature, before I saw this patient, he had formed and got rid of no fewer than fifty-one such concretions."

We might easily quote other interesting cases from this little work. But we have devoted space enough to it, and we have shown our readers glimpses of the valuable material which is interspersed between the statistical tables. As a whole, it is remarkably well done. The author has evidently written it *con amore*, as an illustration of the method of noting cases that he recommends, and with the same intelligence which characterises the surgical work of which it is the record. He seems to quit the subject with reluctance, as if he would have been



glad to have dwelt upon his cases somewhat more in detail; and we, too, have laid aside the volume with regret. In these days of bookmaking it is a pleasure to meet with anything so terse, so sound, and so instructive as this little work.

### IX.—The Modern Treatment of Stricture.<sup>1</sup>

THE almost simultaneous publication of Sir Henry Thompson's works in French, and of two works by American surgeons of note, who have made the diseases of the genito-urinary organs their especial study, gives us an opportunity, which we think may be turned to good advantage, of contrasting the views held by various surgeons on many important questions connected with these disorders. Sir H. Thompson's well-known clinical lectures appear in two forms, though by the same translators, MM. Hue and Gignoux, of Rouen, who have prefaced their own edition with a lengthy anatomical treatise, which is

<sup>1</sup> 1. *Diseases of the Urinary Organs, including Stricture of the Urethra, Affections of the Prostate, and Stone in the Bladder.* By JOHN W. S. GOULEY, M.D., late Professor of Clinical Surgery and Genito-urinary Diseases in the Medical Department of the University of the City of New York, Surgeon to Bellevue Hospital, &c. With 103 wood engravings. New York and London, 1873, pp. 368.

2. *A Practical Treatise on the Surgical Diseases of the Genito-urinary Organs, including Syphilis, designed as a Manual for Students and Practitioners, with Engravings and Cases.* By W. H. VAN BUREN, A.M., M.D., Professor of the Principles of Surgery, with Diseases of the Genito-urinary System and Clinical Surgery, in Bellevue Hospital Medical College, &c.; and E. L. KEYES, A.M., M.D., Professor of Dermatology in Bellevue Hospital Medical College, Surgeon to the Charity Hospital, Venereal Division, &c. London, 1874, pp. 672.

3. *Traité Pratique des Maladies des Voies Urinaires.* Par Sir H. THOMPSON, F.R.C.S., Professeur de Clinique Chirurgicale et Chirurgien à University College Hospital; Chirurgien Extraordinaire de S.M. le Roi des Belges, &c., Traduit avec l'Autorisation de l'auteur et Annoté, par EDOUARD MARTIN, EDOUARD LABARRAQUE, et VICTOR CAMPENON, Internes des Hôpitaux de Paris, Membres de la Société Anatomique; précédé des Leçons Cliniques sur les Maladies des Voies Urinaires, Professées à University College Hospital, Traduites et Annotées, par le Docteurs JUDE HUE and F. GIGNOUX. Avec 280 figures. Paris, 1874, pp. 775.

4. *Leçons Cliniques sur les Maladies des Voies Urinaires, Professées à University College Hospital de Londres.* Par Sir HENRY THOMPSON, Chirurgien Extraordinaire de S.M. le Roi des Belges, &c.; Traduites, Annotées et Augmentées d'une Introduction Anatomique, par les Docteurs JUDE HUE, Ex-chirurgien en Chef de l'Ambulance Internationale Rouennaise, &c., et F. GIGNOUX, Ancien Interne de l'Hôpital Saint Éloi, &c.; Ouvrage contenant 40 gravures sur Bois et 3 Leçons de plus que la Troisième et Dernière édition Anglaise. Paris, 1874, pp. 530.

5. *Du Traitement des Rétrécissements de l'Urethre par la Dilatation Progressive.* Par T. B. CURTIS, Docteur en Médecine de la Faculté de Paris, &c.; Travail couronné par le Commission du prix Civiale, 1872. Paris, 1873.

6. *Cases of Stricture.* By W. F. TEEVAN, 'Transactions of the Clinical Society of London,' vols. iv, v, and vi, 1871, '72, '73.

not, perhaps, an appropriate introduction to an author who in his second lecture advises his pupils "in passing an instrument to forget all about their anatomy." The same want of unity strikes us in the translation of Sir H. Thompson's three works by MM. Martin, Labarraque, and Campenon, under the title of '*Traité pratique des Maladies des Voies Urinaires*,' to which the clinical lectures are prefixed, for here we find the old chapter on the anatomy of the urethra, in the opening sentence of which we are told that "it will be essentially necessary, in order to understand fully and clearly the subject of this essay, to study closely the anatomy of the healthy male urethra."

Sir H. Thompson is well known to have been a convert to the use of soft bougies, which he still persists in calling "bulbous-ended," though his translator gives the proper term, "bougie olivaire." In his second clinical lecture he perfectly openly explains his change of opinion, and says:

"No patient will ever allow a surgeon to pass for him a solid instrument if you have passed for him a flexible one as easily as you are bound to do. It gives him so much less pain and produces so much less irritation."

On the other hand, Dr. Gouley says:

"In my own practice I use the gum bougies almost exclusively up to No. 7, but having reached that number I usually set aside the elastic instruments to use the smoothly polished conical steel sounds. Patients, in taking note of the change, almost always speak of the *slight pain they feel during the passage of the metallic as compared with that produced by the gum instrument.*"

Dr. Van Buren also says (p. 113):

"Less harm can be done with flexible than with solid instruments undoubtedly, and on this account they are to be recommended for the unskilled, and for all, however expert, in the low sizes below No. 9. In trained hands, however, the steel sound is perfectly safe; it is smoother than any soft instrument, and certainly can be passed into the urethra *with less pain than any other instrument.* Patients tested at the same sitting, with soft and steel instruments, *almost invariably complain less of the latter.*"

Thompson's practice is diametrically opposed to that of our American authors, for while they use metal instruments for the larger sizes only, he has recourse to the silver catheter for tight strictures. At page 166 of the French translation he lays down that, failing success with an elastic catheter, "the instrument most especially useful in a case of real difficulty resulting from a very tight stricture is a very small silver catheter, with a shaft less small and of a certain resistance;" and then follow the old directions of the English edition for trying to find the orifice of



the stricture by patient manipulation and direction of the instrument. Again, in a case of extreme difficulty, the probe-pointed catheter is recommended very strongly as of old. Now, the objection to a conical or probe-pointed (which is only a variety of conical) catheter is, that when, as so often happens, there is more than one stricture, the instrument becomes, owing to its shape, fixed in one stricture, so that the point cannot be manipulated into another, whereas a fine instrument of the same calibre throughout can be passed through a series of contractions almost as readily as through one.

Although the American surgeons habitually employ the smallest-sized elastic instruments, yet both Gouley and Van Buren recommend in difficult cases the employment of rigid instruments, viz. whalebone bougies of very minute size. These are two feet in length, and, being more springy, are said to be more readily introduced than the catgut bougie, which used to be employed by the late Mr. Solly and others. If the bougie enters the bladder it is to be used as a guide for a catheter after the manner of Wakley's instruments; but both our American authors curiously enough prefer to introduce a metallic conical "tunnelled" sound to dilate the urethra, and Gouley takes credit for having "in 1869 adapted the tunnelled principle to the one-eyed English gum catheters, and has used them with much satisfaction and success." That is, he cuts off the end of an ordinary elastic catheter, and passes the whalebone guide through the open end and out of the eye, and thus guides the catheter into the bladder. It is obvious that this is not so good as simply slipping an open-ended catheter over the guide, which then lies inside the catheter, and does not tend to fill up the urethra, as in the other method.

Mr. Teevan has been the apostle of filiform bougies in this country, and has read papers on the subject before the medical societies. His cases are not, however, of a very convincing character. Thus the case recorded in Vol. IV of the 'Clinical Society's Transactions' is that of a man with a tight stricture, on whom Mr. Teevan "tried for half an hour to pass various kinds of the smallest filiform bougies without success." Two days after, another attempt of half an hour's duration was made with the same result. Two days later complete retention had come on, and after a quarter of an hour's trial the "smallest No. 1 filiform bougie" was passed into the bladder, and on its withdrawal a very fine stream of urine followed, so that the bladder was emptied in the course of an hour. Again, in Vol. V of the same 'Transactions' we find that Mr. Teevan "had already vainly tried every other day for a fortnight, previously to the patient's admission, to pass a fine bougie;" and that

after the patient's admission to St. Peter's Hospital he "made six unsuccessful attempts, each of half an hour's duration, to introduce the smallest filiform bougie;" and, finally, had to cut the patient without a guide. So also with the whalebone bougie employed on another patient, who was admitted to the hospital on 11th February. It was not until the end of March that the house-surgeon, who had been "passing one of the smallest olivary whalebone bougies as far as it would go" every other day, "got completely through the first stricture, which was two inches long, down to the second one, in the perinæum." And, again, in a third case, from the same series, after repeated efforts to pass "various small instruments," Mr. Teevan had recourse successfully to No.  $\frac{1}{2}$  silver catheter; and although he mentions that this case was "the only one in which he had ever passed a silver catheter, after failing to effect the object with filiform and whalebone bougies," we cannot but suspect that a fine silver catheter might have cut all the cases short, and have saved the occurrence of dangerous retention in one of them.

Supposing an instrument to have been introduced through a stricture into the bladder, when shall it be withdrawn? Thompson is very decided on this point, and recommends immediate withdrawal; and Dr. Curtis, who has entered into the whole question of dilatation most elaborately and ably, coincides, to a great extent, in this opinion. He, however, quotes M. Guyon, of the Necker Hospital, on the other side, as leaving an instrument in the urethra from five to ten minutes, in certain difficult cases. Thompson maintains that dilatation is purely mechanical, and, though not denying absorption, says that "there is not the smallest particle of proof to support the notion." It is difficult, however, on any other theory to explain his statement a few pages later, viz.—"If you leave a No. 1 in for a sufficient length of time, you will be able, when you take it out, to pass No. 10." He takes credit to himself for having been the first person to demonstrate this "curious fact." But is it a fact? We have never been able to satisfy ourselves that it is, and we find no reference whatever to it in other authors, particularly Curtis, who has discussed dilatation in all its varieties with the minutest care. Continuous dilatation by a succession of instruments is allowed on all hands to be a satisfactory mode of treating many obstinate cases. The advantages and disadvantages of the two methods of gradual and continuous dilatation are summed up by Curtis as follows:

1. *Gradual or simple dilatation*, when it is applied according to the indications and after the rules laid down, is a good treatment for simple strictures, of more or less recent origin and not



traumatic, nor having been submitted to previous treatment. It gives, moreover, very satisfactory results in a certain number of cases which do not fulfil all these conditions, especially if the bougie is allowed to remain for from five to fifteen minutes, instead of being removed immediately.

2. It is almost completely harmless when employed according to rule.

3. The duration of the treatment, averaging twenty-eight days, hardly exceeds that of other methods, particularly if one takes account of the preparatory and after treatment which these almost always require.

4. This proceeding has the great advantage of not confining the patient to bed; he can continue his business while under treatment, and need only visit his surgeon every second or third day for a few minutes; the workman can attend the hospital without interrupting his work, and without its being necessary to give him a bed.

5. Dilatation no more pretends to cure strictures radically than other methods of treatment do; all must be followed by after-treatment indefinitely prolonged; all are followed by more or less rapid relapses when the after-treatment is neglected.

6. The curative effects of gradual dilatation do not show directly, but they develop gradually; consequently dilatation is not suitable for cases in which it is of importance to re-establish the calibre of the urethra immediately. In such cases urethrotomy or divulsion ought to be preferred.

7. Gradual dilatation is powerless in irritable strictures; it ought to be abandoned after the help of certain degrees of retention of the bougie has been tried; it is equally useless in elastic or resilient strictures.

8. The proceeding may do harm in certain cases, especially those complicated with renal disease. Internal urethrotomy is then more generally suitable, and is at once the best treatment both for the stricture and the kidney affection.

9. *Continuous dilatation* is a proceeding which is unsuited for general application.

10. It has none of the advantages of the other method, neither its relative harmlessness, nor its efficacy as regards permanency of result. This treatment is also as trying to the patient as the more radical methods, and ought not therefore to have the preference over them.

11. Continuous dilatation has the advantage of producing dilatation rapidly and surely, though one cannot always count upon maintaining the result, the relapse is so rapid. Thanks to this last advantage, this method answers sometimes in the treatment of strictures where gradual dilatation has failed;

associated with other methods of treatment it may be of service either at the beginning or in the course of gradual dilatation—in very difficult or irritable strictures; or as a preparation for internal urethrotomy or divulsion.

Divulsion, or splitting a stricture, is the practice advocated by Mr. Barnard Holt, under the term “immediate treatment,” and has been received with much favour in America as well as in this country and abroad. Mr. Holt’s is undoubtedly, and has always been acknowledged by him to be, a modification of Perrève’s instrument; but it is the method of employing it which is due to the English surgeons, as is clearly and fairly stated by Gouley (p. 60):

“In studying M. Perrève’s cases, published in his treatise of 1847, it will be noticed that in every one the occurrence of hæmorrhage is reported; sometimes only to the extent of a few drops, generally more, depending upon the size of the rod introduced. M. Perrève did not *dilate*, as he claims, but did *tear* these strictures; hence his success. Mr. Holt was, perhaps, the first to prove that there was laceration instead of dilatation, as he had an opportunity to verify this in one of his own patients at the dead-house, when there was found a longitudinal rent along the floor of the urethra at the seat of disease. Extensive experience has since taught the profession that nothing short of complete divulsion of the stricture will insure success, and that after all it amounts to about the same thing as internal urethrotomy; but it is a safer operation. Therefore Sir Henry Thompson’s precept of *stretching* as much and *tearing* as little as possible might well be reversed, and I will venture to say *tear* as much and *stretch* as little as possible, and the results will be all the better.”

Van Buren, too, speaks in much the same terms, and says:

“The operation of divulsion, blind and rough and brutal as it appears at first sight, has proved itself exceedingly mild in its immediate and satisfactory in its ultimate results.”

On this latter point Sir H. Thompson is at variance with Van Buren, for he says:

“My principal objection to it (Holt’s method) is that the benefit obtained is certainly not very enduring in severe cases of stricture, and it is for such only that operative measures are necessary. I cannot say much more for the forcible over-distension proposed by myself, and now practised much—and, as it would appear, successfully—in America.”

This plan of “over-distension” was brought out as a sort of “counter-blast” to Holt’s dilator, and, as may be seen in the report of the meeting of the Royal Medical and Chirurgical Society, before which the original paper on the subject was read (‘Lancet,’ May 9, 1863), its object was “not to rupture



but to over-distend the fibrous tissue which constitutes the stricture, so as to destroy, or at all events to impair, its natural tendency to contract." The American surgeons, on the contrary, have greatly increased the strength of Thompson's instrument, and openly call it a "divulsor," and Van Buren says (p. 114):

"Thompson's idea of the proper use of this instrument (as expressed by his naming it a rapid dilator) was, that it should stretch as much as possible without tearing. To obtain the greatest usefulness from the instrument, however, this idea must be abandoned; on the contrary, it should be used with the avowed object of rupturing (divulsing) the stricture. In this way only can its full and best effect be obtained, and, so employed, it is the best instrument we possess for performing divulsion."

In a note Van Buren states, on Bumstead's verbal authority, that "Thompson has recently given up the use of the instrument," and this we have ascertained to be the fact.

That there are cases of stricture so elastic that they can be readily stretched up to any size, and then immediately re-tract, is well known to all experienced surgeons, and Van Buren gives an example. It is extremely difficult, well-nigh impossible, to split them by any instrument, and they are best treated by internal division. The operation of divulsion is acknowledged on all hands to lead to singularly little constitutional disturbance, but it has never been supposed to "cure" strictures, which always require careful after-treatment for an indefinite period. The cases, therefore, published by Mr. Teevan in Vol. VI of the Clinical Society's Transactions of "bad result" after neglect of the subsequent treatment, have really no bearing on the question. We conclude this portion of our subject by quoting the concluding sentence of Gouley's very interesting and elaborate chapter on the question (p. 75):

"I believe divulsion possesses great advantages over cutting operations in properly selected cases, and that it will be often resorted to where internal and external division have heretofore been employed; but it can never entirely supplant these and other methods, though it will probably continue to occupy a very prominent and important position among the surgical therapeutics of stricture of the urethra."

The operation of "internal division," in the modern acceptation of the term, has little in common with the scarifications once practised, and still less with the thrusting of a sharp blade at haphazard against an obstruction in the hope of dividing the stricture. Stafford's "lancetted stilette" was of this latter class, and deservedly fell into disuse. Strictures near the meatus may be readily divided with a straight *bistouri caché*, or with Fergusson's long director and straight knife, but strictures

further down may be divided either from before backwards or from behind forwards (retrograde urethrotomy of the Americans). Thompson gives his unqualified support to the latter method, and prefers the simplest form of instrument, *i. e.* Civiale's, on the very reasonable ground that "with but little mechanism it responds like a scalpel to the hand; in fact, it is merely a long-handled knife." Gouley employs both methods, and gives a series of fifteen cases treated by both plans and by different instruments. He employs a modification of Maissonneuve's urethrotome, which is a favourite instrument in France, and consists of a guiding rod with a sliding knife, which can be adapted to either the concavity or convexity of the instrument. It is fitted with a conducting bougie, which shows the way into the stricture, and lies curled up in the bladder, but the fact is that no urethrotome should be employed until the stricture is sufficiently dilated to admit it easily. Maissonneuve's instrument, even with Voillemier's guard, is very apt to incise the healthy urethra extensively, and, moreover, the conductor may lead it into false passages. Whatever the method employed, a clean cut through the floor of the stricture is the point to be aimed at. The difficulty in practice is to do enough and yet not too much, for we have known severe hæmorrhage and extravasation of urine occur in the most skilful hands. Of course, subsequent dilatation must be carried out, and Dr. Otis, of Philadelphia, has contrived a combined dilator and urethrotome which would be very serviceable in those cases of elastic stricture which cannot be ruptured on account of their yielding quality. It is claimed for internal urethrotomy that it gives more lasting results than any other operation.

The term "external urethrotomy" ought, as maintained by Thompson, to be confined to an operation performed upon a guide passed through the stricture, *i. e.*, Syme's operation, and must not be confounded, as it so frequently is, with perinæal section, *i. e.* laying open the urethra through the perinæum in cases of impassable stricture. Our American authors speak of "external perinæal urethrotomy *without* a guide," and "external perinæal urethrotomy *with* a guide," but they are only two varieties of perinæal section, though the terms are not used in quite the same way by the two authors.

As Van Buren justly remarks: "Few operations in surgery are more formidable than this one of external perinæal urethrotomy *without a guide*." It consists in passing a blunt staff or catheter down to the stricture, and then cutting beyond it and laying open the perinæum, until the healthy urethra is reached, when the catheter is passed on into the bladder. This operation used to be recommended for cases complicated by retention



of urine, and we have seen most accomplished surgeons come to utter grief in attempting it. Of late years it had, we thought, been completely abandoned in this country, but we find in Vol. V of the Clinical Society's 'Transactions' (1872) two cases of the kind recorded by Mr. Teevan, who says he has performed the operation seven times. Of the two cases thus recorded, in one the operation was undertaken for retention, and in the other for the treatment of a complicated and impassable stricture. Mr. Teevan thus sums up:

"For the performance of this, the old French operation of 'la boutonnière,' it is absolutely necessary that the light should be good and an able assistant at hand; it is an operation not nearly so difficult as some suppose, for as it is now a well-known fact that there can be no stricture in the prostate, we have only to dissect down to that organ to enable a catheter to be passed into the bladder. I have performed the operation seven times, and in no instance have I failed to reach the bladder. I do not for a moment pretend to have in these cases divided the stricture, and that only. What I did was to divide the diseased structures in the median line in the natural course of the urethra, and I have no doubt but that in several cases the urethra was, when restored to its natural calibre, an entirely new canal in the part where the operation was performed."

It is a little difficult to reconcile the last sentence with a violent attack upon Syme's operation for impermeable urethra, contained in the same page, and which runs as follows:

"My objection to Syme's operation is that it involves the formation of a false passage, or a laceration of the urethra. Now, if there is one fact better known than another, it is that the worst form of stricture is the traumatic—that caused by laceration—and hence Syme's operation is opposed to all the teachings of surgery and pathology"!

External perineal urethrotomy with a guide, according to Goulay, is best done by passing a series of whalebone bougies down the urethra and into all the false passages, so that finally one of the bougies finds its way into the stricture. Over this he slips his tunnelled catheter staff, the point of which is brought against the stricture, and serves as a guide for opening the urethra from the perinæum. The edges of the canal are held asunder with loops of silk, as recommended years ago by the late Mr. Avery, and also by Thompson; the stricture is divided, and the catheter staff pushed on into the bladder. In the much more difficult cases of totally impassable stricture (in which alone the operation is really required), the urethra having been opened in front of the stricture, a diligent search must be made with fine probes, or directors, for the orifice of the stricture, and the division effected. The curved concave director recom-

mended by Thompson to be passed along the staff when in the bladder, so as to serve as a guide for the catheter, is very convenient and useful, but, of course, not required if a "catheter staff" is employed, which instrument, by the way, is no novelty, having been invented by Mr. Jonathan Hutchinson many years ago. It is the custom of most surgeons to leave a catheter in the bladder for forty-eight hours after perineal section, but Goulay argues strongly against the practice, believing that it can do no good, and is the cause of much evil, including rigors, fever, &c.

The discussion of the treatment of stricture naturally leads to that of retention of urine, as one of its results. Too much stress cannot be laid upon Thompson's *dictum*, that "an involuntary flow of urine indicates retention, not incontinence," for mistakes on this score are of too frequent occurrence. It may be disputed whether the cause of the obstruction is congestion or muscular spasm, or, more probably, both; but there can be no doubt that retention occurs in comparatively healthy urethras from constitutional causes. That such cases may be relieved *in time* by the administration of opium and the warm bath, and the abstraction of blood from the perinæum, is a well-known clinical fact, but the whole pith of the matter is well summed up by Thompson in one sentence:

"With a moderate-sized flexible catheter there is generally no difficulty in relieving the patient, who is exceedingly grateful for what you have done; whereas if you put him through the long process, and he relieves himself ultimately, he thanks you for little, comparatively speaking, and he runs the risk to which I have referred (atony of bladder from over-distension)."

Having already discussed the merits of flexible and metallic catheters in cases of organic stricture, we need not enter upon that subject again in connection with retention, but pass on to the question of treatment of retention from organic stricture when all attempts at catheterization have failed. Forcible catheterization is universally tabooed, though we have known two patients thus slain by a leading surgeon, and we have already discussed the various forms of perineal section suitable to such an emergency. There remain, then, only puncture of the bladder by the rectum or above the pubes, and Mr. Cock's modification of the boutonnière operation by opening the distended membranous urethra behind the stricture from the perinæum.

Mr. Cock, of Guy's Hospital, has been the great promoter of the rectal puncture of late years, and Mr. Bryant, in his recent 'Manual of Surgery,' corroborates all Mr. Cock's teachings. The operation is certainly a simple one, and if the bladder is



distended the surgeon can hardly go wrong. Thompson and the American surgeons evidently regard the operation with no particular favour, and apparently think every difficulty may be overcome by careful catheterization. This is, to a great extent, true where practice and skill are combined, but when, as must occur, a surgeon but little adept with the catheter is brought face to face with a case of retention urgently demanding relief, we believe he will do well to have early recourse to puncture per rectum. Mr. Cock's operation of opening the membranous urethra is applicable either for the relief of retention or in order to divert the urine from a strictured urethra riddled with false passages, and is also highly lauded by Mr. Bryant, whose work may be consulted for a detailed description of the proceeding.

The modern operation of "aspiration," in connection with retention, is referred to by both our English and American authors, whose experience of it appears, however, to be very limited. Although it may be employed to give temporary relief, inasmuch as it in no way tends to ameliorate the patient's ultimate condition, it seems hardly likely to displace other and more received methods of treatment.

### X.—Recent Works on Fever.<sup>1</sup>

It seems to us to be a very healthy sign of medical publication that three such important and in many respects such diverse works on fever should be issued so close together from one publishing house; it is not only a sign of liberality on the part of the publishers, but, what is far more important, it is a proof of the liberality of mind existing in the profession. For if the professional mind were restricted to this groove or to that, and did not seek truth impartially wherever there was any reason for expecting to find it, no publisher would be so lost to self-interest as to spend money in promulgating views in any respect opposed to those generally received. And this impartiality of the professional mind in its search after truth is the surest guarantee for the safety of the public who entrust their happiness and their lives to those actions which are bred of its decisions. In a world like the present, where probability alone is attainable, and where, in a science like medicine, the bases of that probability vary so infinitely, it is only by earnestly and constantly learning that we can expect hopefully either to

<sup>1</sup> 1. *A Treatise on the Continued Fevers of Great Britain.* By CHARLES MURCHISON, M.D., LL.D., F.R.S., &c. Second edition. London, 1873, pp. 729.

2. *Typhoid Fever; its Nature, Mode of Spreading, and Prevention.* By WILLIAM BUDD, M.D., F.R.S. London, 1873, pp. 193.

3. *Lectures on Fever.* By WILLIAM STOKES, M.D., D.C.L. Oxon., F.R.S. London, 1874, pp. 459.

teach or to practise medicine. In the exercise of medical criticism we take up, however, a somewhat different position ; and though not disdaining to be taught, it is yet our duty to keep ever before us as a standard the present position of medical opinion in regard to the matters in hand ; and though our work may be judged of from many points of view, it will yet be found to be most useful according as we have been able truly to represent that opinion, accurately to trace the divergencies from that opinion in the works before us, and to point out the tendencies of those divergencies, whether progressive or retrograde.

Now and then there come before us works which are so true an embodiment of the present state of medical opinion in regard to the subject treated of that they are felt to be beyond the pale of true criticism, to be, in fact, themselves critiques on the subjects treated of on a more extended scale than we could find room for, and with more ample illustrations than we could supply. Dr. Murchison's work is of this character ; we formerly characterised its first edition as comprising a "most conscientious and painstaking inquiry into the identity or non-identity of the poisons producing the different forms of continued fever, an extended and laborious historical summary of the various epidemics which have been chronicled by successive generations of physicians, and an essay on etiology, geographical distribution, phenomena, diagnosis, prognosis, and treatment, which for scope, accuracy, and research, has seldom been surpassed ;" and we then concluded with the words that "the publication of such a book marks an era in pyretology."<sup>1</sup> Eleven years have passed since these words were penned, and now a second edition of this work lies before us, enlarged by about one hundred pages, much of it rewritten, and comprising the entire medical history of the London Fever Hospital from the period when the different forms of continued fever were first differentiated from one another (1848) till, from the transfer of the pauper fever patients to the new fever asylums in 1871, the experience of the London Fever Hospital ceased to be any accurate test of the prevalence of the different forms of fever in the metropolis. The statistical tables in the first edition were based on 6703 cases of continued fever, those in the present edition on 28,863 cases, of which nearly one half were under Dr. Murchison's personal care. This very large body of facts observed is quite equalled by the enormous number of historical references, which amount to 3000, both together combining to present us with a history of fever unequalled in its comprehensiveness by any other treatise in this or in any other language. We shall presently have occasion to refer more at large to various parts of the contents of this most admirable work.

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<sup>1</sup> 'British and Foreign Medico-Chirurgical Review,' July, 1863, p. 119.



Dr. Budd's very handsomely printed and illustrated monograph is a most valuable contribution to the history of typhoid fever. It embodies more than thirty years' special experience of a form of disease which annually carries off more than 15,000 victims, while during the same period over 140,000 pass through the protracted miseries of the disease—miseries which no one can in any measure realise who has not had experience of this fever in his own home, and the full intensity of which no one can fully appreciate unless he has not only seen this disease in the homes of the rich, but has also observed it in the cottages of the poor, who have but scant provision for the bare necessities of life, and none at all for its great emergencies. "How often," says Budd, "have I seen in past days, in the single narrow chamber of the day labourer's cottage, the father in his coffin, the mother in her sickbed in muttering delirium, and nothing to relieve the desolation of the children but the devotion of some poor neighbour, who in too many cases paid the penalty of her kindness by becoming herself the victim of the same disorder. In its ordinary course life has few such consummations of misery as this."

The emotions are not good guides in matters of scientific inquiry, but where life, death, and the happiness of mankind are concerned, they may at least be permitted to stimulate investigation; and it is extremely honorable to Dr. Budd that his anxious desire to relieve the miseries of which he was so often a sorrowful witness has led him to devote the best part of his life to the discovery of means by which similar calamities could be prevented, the result being the production of this work, every page of which is full of burning words—"full of rage, and full of grief." It appears to us a most remarkable fact that Dr. Budd, who was educated long before the various forms of continued fever were differentiated from one another, only once alludes, and that in the most casual manner, to the possibility of typhoid fever being confounded with typhus. Dr. Budd obviously has himself no difficulty in accurately differentiating the one from the other, and he sees so little reason for any one having any difficulty in doing so that he talks of typhoid fever as any one does of such generically specific diseases as pneumonia or smallpox, only remarking, in a note referring to the pauperism produced by fever, that, as the returns referred to the year 1838, a certain proportion of these fever cases were probably typhus, as before that typhoid and typhus fevers were confounded with one another, and registered as one disease by all but a few scientific men. The whole of Dr. Budd's work is, indeed, founded upon the assumption that typhoid, or, as we prefer to call it, enteric fever, is a generically specific disease,

which propagates itself and propagates no other kind of fever. "In the numerous cases," he says, "in which I have seen the disease palpably spreading by contagion, the offspring has always borne the same specific marks which distinguish the parent; and one case has followed another with the same constancy of specific type with which smallpox follows smallpox, or measles succeeds to measles." This strong and decided opinion as to the specific distinctness of enteric fever, the result of a lifetime devoted to the close and accurate observation of all the relations of this special disease, with the view of ascertaining the possibility of its mitigation or prevention, is not, however, that held by all the profession, as we are reminded by Dr. Stokes in his interesting and in many respects instructive lectures. In stating his views Dr. Stokes is just as strong and just as decided as Dr. Budd, and they may be put even more tersely in the words of Alison than in those he himself has used. Alison says, "It is certain that some cases resulting from the contagion of the usual spotted typhus show all the symptoms and post-mortem appearances attributed to the typhoid fever, and therefore it seems most probable that the differences observed are only varieties depending on constitution, and on the agency of other causes affecting the constitution, besides the exciting cause of the disease."<sup>1</sup> This is the essence of what Stokes has said in various places throughout his lectures; quoting his own words will scarcely make this plainer: "Fever," he says, "has been somewhat arbitrarily divided into two classes, or placed under two great heads—*typhus* and *typhoid*;" and after describing the symptoms and supposed modes of origination of these two forms, he goes on to say—

"We may have typhus without measly eruption or maculæ; we may have typhoid without diarrhœa, rose spots, or any other symptoms said to be pathognomonic. We are compelled to admit that pathological anatomy has failed as a means of pointing out any essential distinction between these two forms of fever. Anatomy, it is true, may reveal to us certain morbid changes and abnormal conditions in different organs in many cases of either typhus or typhoid. But these alterations are only the results, and not even the necessary or constant results, of the primary disease; and so they throw no light upon the object of our search—a vital distinction between the two fevers."

Less prejudiced, however, and therefore presumably more accurate inquiry, compels us to admit that, so far from the lesions found post-mortem not being the necessary and constant results of the primary disease, the reverse is the case.

"The records of the London Fever Hospital tell a very different

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<sup>1</sup> 'Outlines of Pathology and Practice,' 1834, p. 447.



tale; during twenty-three years upwards of a thousand autopsies have been made of patients dying of fever, and amongst them there was not one single exception to the facts that, when the eruption of enteric fever—*i. e.* lenticular rose-coloured spots coming out in successive crops—had appeared in the course of continued fever, the abdominal lesions of enteric fever were invariably present, while when the eruption of typhus, characterised by what Jenner has termed a mulberry rash, numerous spots, irregular in size and form, isolated or grouped together, often closely resembling the eruption of measles, at first slightly elevated above the skin, and of a florid or dirty-pink colour, and disappearing on pressure, becoming after the second day persistent, dingy, reddish-brown stains, no longer elevated above the skin, and only paling, not disappearing, on pressure, without defined margin, and accompanied by a deeper-seated, paler eruption of a similar kind, which has been termed the subcuticular mottling—when an eruption of this character occurred during continued fever the abdominal lesions of enteric fever were, according to these records, invariably absent. Moreover, since 1861 the patients in the London Fever Hospital with typhus have been treated in different wards from those with enteric fever, and in nine years (1862–70) there were admitted 14,589 cases of the former disease and 3558 of the latter, but only in two instances, and these very doubtful, was there any suspicion of the two diseases comingling” (Murchison, p. 666).

The experience of the London Fever Hospital is therefore entirely against there being any gliding of the one form of fever into the other, and completely in favour of there being two distinct forms of fever, each with an entirely different clinical history and a perfectly distinct series of anatomical lesions, and this is supported by the experience of others. Dr. Murchison quotes among many others Dr. W. T. Gairdner, who states in 1860 that, after a most careful personal inquiry, he could discover among the fever cases occurring in Edinburgh during the previous summer nothing to lead him to suppose that in any case “typhus had given rise to anything but typhus, or enteric fever to anything but enteric fever.” We need not multiply quotations to the same effect; this part of the controversy is well known, and has been fully entered into in our former review of Murchison’s first edition. It resolves itself very much into a question of diagnostic ability and of specific clinical education.

A study of the history of the controversy as to the identity or non-identity of typhus and enteric fevers brings this out very clearly. Dr. Henry Kennedy, one of the most strenuous advocates for their identity, states that in one of his cases the spots thought to be characteristic of enteric fever were indistinguishable from true petechiæ on the fourth day of their appearance.

Huss' "taches roses lenticulaires" includes all the spots met with in fever which are neither petechiæ nor sudamina, includes therefore the eruptions met both with in typhus and enteric fevers, because petechiæ and sudamina are met with in both, and are peculiar to neither. Unless, however, we have clear and distinct ideas as to the nature and course of the eruptions in the two forms of fever, which are now very well known, and unless we are able to describe them more accurately than either Kennedy or Huss has done, it is idle to argue the matter from the eruption point of view. We have frequently seen both forms of eruption when scanty, overlooked, or mistaken, and yet had no difficulty in getting them duly recognised when their characteristics were properly pointed out. It is the same in regard to the anatomical lesions; Kennedy admits that he has found it impossible to distinguish between the lesions of enteric fever and those of tubercular disease of the intestines, and Dr. Joseph Bell, of Glasgow, maintained that there was no such thing as typhus without the intestinal lesions of enteric fever, though he admitted, or rather asserted, in explanation, that these lesions were frequently so insignificant as sometimes to require a lens for their discovery. To mistake the lesions of enteric fever for those of tubercle, or to assert that their discovery requires a lens, is simply a confession of such entire ignorance on the subject of the pathological anatomy of enteric fever that it would be futile to attempt to correct it here.

When, however, Dr. Stokes asserts that "similar exciting causes seem occasionally capable of giving rise to either form of fever" (preface, p. xi), and when he further extends this proposition by saying—

"When fever appears in a family living in some confined situation in a large city or town, in a badly ventilated dwelling, perhaps in the midst of an unwholesome and densely populated neighbourhood, several members may be struck down by the disease. They may sicken simultaneously or one after the other, so that we are afforded the opportunity of witnessing the effects of the malady on them individually. Under such circumstances it has been observed that a marked variety is presented in the condition of the several patients. One will have the disease in its severest form, another will experience but a mild attack; some will suffer from protracted fever, others will go through an illness of the briefest duration; one will have petechiæ, another will present no eruption; one will display critical phenomena, whilst another will recover without crisis of any kind; one will have typhus, another typhoid, or even *rheumatic fever*"—

he goes, we think, a long step further in the doctrine of essentialism in fever than will probably be approved of, even by



those who believe in the identity of typhus and enteric fevers; and yet he is only, we think, pushing his own opinions to their legitimate conclusion; whether that conclusion be likely to be useful to mankind is quite another matter. It is quite characteristic and consistent with these views, though it is somewhat astounding in the present year of grace, and after all that has been written and said upon the subject, to find Stokes stating, with the utmost simplicity and the most perfect good faith on his part, "In 'relapse cases'—I do not mean cases of relapsing fever—the character of the second attack may differ widely from that of the first. Thus, typhus may give place to typhoid, or *vice versa*" (preface, xi).

If we grant this, there can be no difficulty, we think, in admitting the possibility of the second attack assuming the form of scarlatina, of rheumatic fever, or even of smallpox.<sup>1</sup> In favorable circumstances the "relapses" may glide into any of these varieties of fever. Rheumatic fever, it may be said, is not contagious, and does not usually present any of the characteristics of a specific fever; but the same may still be said of an ordinary febricula, which Tweedie regards as a mild form of typhus, and was held till lately by many to be true of enteric fever. Moreover, in all such fevers there are the utmost differences in regard to contagiousness, some being very virulent, others so little contagious as to be considered free from it. There are also the greatest differences in regard to contagion or power of spreading which is equivalent to contagion in each febrile type at different times and in different places, or in the same place at different times and seasons; thus, typhus prevails for the most part in great and wide-spread epidemics occurring at long intervals of time, sporadic cases occurring between these epidemics. Again, enteric fever prevails with more or less intensity every autumn, or after prolonged heat and dry weather, though sporadic cases are never absent from any large town, while relapsing fever only appears at long and uncertain intervals, and seems to vary less than any other form in regard to its contagious properties; and febricula is said by Murchison to be "independent of any specific poison, and, therefore, not contagious." We rather fancy the argument would, from his point of view, be better put the other way; febricula is not contagious, and, therefore, is not the product of any specific poison; but, indeed, we do not see why there should be a *therefore* in any case; we can conceive of happier auspices under which

<sup>1</sup> In various places Stokes acknowledges that rheumatic and typhus fever arise from the same poison, and thus the poison of continued fever may be supposed to glide into that of the exanthemata through scarlatina, rheumatism and dengue, on the one hand, and by the assumed connection between typhoid and smallpox on the other.

relapsing fever might continue to perpetuate itself, and even febricula ripen its germs. Is it not probable that the same climatic and telluric causes which render febricula non-contagious, and which at one time limit and at another favour the spread of other indigenous forms of fever, are closely allied with those which stop the spread of yellow fever beyond the forty-eighth degree of north latitude, and which almost confine it beneath the level of 2500 feet above the sea, as well as with those which limit the spread of plague, even in plague countries where certain closing days are anxiously longed for, these being for Egypt the feast of St. John, June 24th; for Smyrna the feast of St. Rochus, August 15th; nearly the same for Constantinople and for Trebizond, the autumnal equinox, &c., after which days the most ardent sticklers for contagion no longer avoid the sick, and though sporadic cases do occur the disease soon dies out. Even intermittent fevers vary with the season, not only in Europe, but also in their native haunts, and so well is this known and felt by all animate creation that in the unhealthy season—from April to October—the fever-haunted Terrai of India is so completely deserted that not so much as a bird is to be seen or heard in the frightful solitude, which at other times resounds with all the ceaseless noise of tropical animal life. Of course, the idea that the origin and spread of all forms of fever are influenced by the same causes involves the belief that such fevers originate in thermal and meteorologic influences, which affect injuriously the telluric and corporeal conditions, and this idea has not wanted able expositors—Dr. Mackintosh, Henle, Oldham, and others; and it is only by holding fast to some such belief that the essentialism of fever, as taught by Alison and Stokes, can be made to assume even the appearance of a philosophic reality.

From this point of view the essential cause of fever is a central force of a somewhat similar importance in medicine to what energy is in physics; far from disavowing it, we think this doctrine not only plausible, but even probable. It has its counterpart in cosmic chemistry, in which spectrum analysis shows us that the spectrum of the most highly heated stars exhibits only the lines indicative of the presence of hydrogen, while in those which are cooling other substances gradually make their appearance, till we arrive at the complex spectra of those cold stars which contain as many ingredients as are known in our own earthly home. From such facts we may infer that there is a basis of probable truth in Samuel Brown's apparently most extravagant conception of the possible transmutation of metals and the actual identity of all our so-called elements. But the beauty and possible truthfulness of this really philosophic idea



never for one instant influences our practical ideas as to the relative value of the metals, nor does it in any way affect our mechanical operations with these metals, and that man would, indeed, rightly be regarded as insane who should permit them to do so. Why should it be otherwise in medicine? Why should our belief in the essential unity of the cause of disease generally, or of any class of diseases in particular, interfere with our rational treatment of its differentiated products? It is not so in physics; motion and heat are the correlatives of each other, visible motion may be converted into sensible heat by impact or arrest, and both may be traced back to one essential cause—energy, yet unity of cause has no influence in our treatment of results in this case; no one would think of arresting the movement of a railway train by directing upon it the stream from the hose of a fire-engine, nor would any one in his senses seek to put out a fire by piling on it logs of wood; reverse these measures, however, and their appropriateness strikes us at once. Neither is the case otherwise in practical medicine in our treatment of the ordinary forms of disease; exposure to cold will produce in one person bronchitis or pneumonia, in another diarrhœa or dysentery, in a third merely toothache; but we never think of employing odontalgics for the cure of bronchitis, and we also modify our treatment according as the lungs or the bowels are the seat of catarrh; and why should it be otherwise in regard to fever? It is possible enough that one essential cause may underlie all fevers. Whether we personify that, and call it the Typh poison, as Dr. Chambers has done, or whether we regard it as the result of temperature or other meteorologic cause acting upon bodies variously modified, the result is the same; it is the effects we treat, and not the cause, and all fevers require a treatment more or less modified according to the differences displayed in those injurious or morbid phenomena which incidentally occur while the organism is righting itself from the influence of that cause, phenomena which do not depend so much upon the essential nature of the cause as upon the condition of the organism. Any attempt to treat fevers upon an opposite plan has resulted in nothing but disappointment; thus, Dr. Dundas, basing his views upon the fact that in tropical countries temperature, whether seasonal or local, is so capable of influencing that cause which we personify by the term malaria, as to produce in the same place at different seasons, or in the same place and seasons at different altitudes, fevers of which the least severe type is intermittent and non-contagious, and the most severe type more or less continuous and apparently contagious, propounded in 1852 his theory that there are no specific differences between inter-

mittent, remittent, and continued fevers, and that all alike are amenable to one remedy—quinine. Yet this treatment has not been found to be so successful as he expected, nor so successful as it would undoubtedly have been had his theory been true. On the other hand, intermittents, as we know from past history, may be recovered from without quinine, and from the success of Dundas and his followers we also know that continued fever may be recovered from under the use of quinine, but the experience of other parties teaches us that antiperiodics, such as quinine and arsenic, are necessary for the successful and rapid cure of intermittents, while continued fevers, though they cannot be cut short in the same summary manner, are yet capable of being more successfully conducted to a close by appropriate phenomenal treatment than by the empirical employment of quinine. While thus the doctrine that the existence of one essential cause of all fevers leads to such a similarity of nature as would cause them all to be amenable to the same treatment produces only results which are detrimental to the interests of mankind, the same doctrine, when similarly applied to the prevention of the various forms of fever, will be found to be equally erroneous, and to lead to much more disastrous results.

The idea that the drainage and settlement of a country has led to the disappearance of intermittent fevers, and their transmutation into those of the continued type, is one which flows from the doctrine of one essential cause for all fevers, but though it contains an element of truth it is not wholly true in either particular. For we learn from the history of Scotland that intermittent fevers disappeared from that country long before there was any improvement in its drainage, this disappearance being ascribed by those best qualified to judge to an improvement in the living (dietary) of those exposed to the malaria (Christison, 'Social Science Address,' 1863), a statement which is also borne out by the fact that those regiments in India who live better than others suffer the least from ague; and we also learn from the history of fever generally that this disappearance of ague was not connected, in Scotland certainly, and we believe elsewhere, with any equivalent increase in any of the other forms of fever. Thus we learn that an improvement in the mode of living in those exposed to its causes prevents the occurrence of intermittents, even while these causes still continue, and that neither the cessation of this form of fever, nor the actual removal of its causes, is necessarily followed by any increase of fever of any other type; and we also learn from the history of fever that, though intermittents never penetrate to the heart of large cities, yet no sooner does destitution and



overcrowding prevail there than we have the development of typhus fever, with or without the concurrence of relapsing fever; and the history of all recent epidemics, more especially that connected with the cotton famine in 1862, teaches us that the prevention of destitution, the securing of better accommodation for the people, and the isolation of the first occurring cases, is a safe and efficacious mode of checking the epidemic spread of such forms of fever, which are not transmuted, but simply extinguished; and there seems, thus, every reason to believe that the unexampled prosperity of this country during the last twenty years has been the great means of preventing the occurrence of those great epidemics of such fevers which used to prevail at uncertain intervals, by affording the masses of the people the means of procuring ample supplies of nutritious food as well as good and suitable house accommodation, while any attempt at the recurrence of typhus during that period from a recurrence of its usual causes has been efficiently checked by the eleemosynary removal of these causes and the isolation of the first occurring cases. On the other hand, it has only been during the last thirty years that enteric fever has been gradually asserting itself as a serious addition to our nosology, till its intestinal phenomena, which were at first only reckoned rare and occasional complications of continued fever, have become almost universally acknowledged to be pathognomonic of a distinct form of fever. During this period we have not, indeed, had any attempted recurrence of intermittent fever on a large scale, because practically its causes have ceased to exist generally; where they still exist, as in some of the southern counties of England, it still continues to be as prevalent as formerly; but when destitution has prevailed, and been inefficiently provided for, we have had some of the most wide-spread and destructive epidemics of typhus which have ever existed; we need only refer to that of 1843, and especially to that of 1846, which was of unprecedented magnitude and duration, to various local epidemics arising from local causes, and to that of 1862, which threatened certain special districts, but was happily cut short by a due attention to the old motto *obsta principiis*, and a more clear understanding of what these *principia* were. But enteric fever has never formed any important part of these great epidemics; it has always prevailed more or less, but it has not been found to increase with an increased prevalence of typhus, neither has it been found to decrease with the decrease of typhus, nor, as some have supposed, has it ever increased to take its place. There is nothing in their histories to show that typhus and enteric fevers have ever been transmuted into one another. On the other hand, the prevalence of enteric fever in

London has been largely increased by the completion of its main drainage scheme. In Edinburgh it has greatly increased from the same cause. By some it has been attributed to defective water supply, but in Glasgow, where the water supply has recently been made as ample as it could well be, this disease has not thereby been lessened. It is true that Hornemann (*'Virchow's Archiv,'* 1871) shows by an elaborate comparison of the mortality tables before and after, that typhoid fever was largely diminished in its prevalence and severity in Copenhagen by the introduction of waterclosets and an increased water supply, but those acquainted with the foul "abtritts," and nearly as foul water supply, of Continental cities, will not wonder at this statement, but rather look forward to a subsequent increase, and that with all the more confidence that he confesses that diphtheria has been introduced along with the water supply—"Diese Krankheit," he says, "zumal ist neu," for we know that the proofs that diphtheria and enteric fever are both dependent upon sewage gases, or what escapes with them, are very much the same. From these facts, therefore, for the full proof of which we must refer to Murchison, as we have no space here to reproduce either his admirable epitomes of the histories of these various forms of fever, or to refer to those many other monographs and papers which amply bear out the conclusions we have stated, we feel warranted in saying that in the past history of fever there is nothing to induce us to conclude that all its forms depend upon one active cause, but rather that their causes are as diverse as their phenomena, and that in regard to any supposed unity of their essential cause there are, at least, as powerful reasons for believing in the essential unity of all metals as in the essential unity of all fevers, and while all mankind would universally acknowledge that no one but a madman would practically act upon the former belief, practical action on the latter would be fraught with infinitely more danger to mankind. Bridges may be built of pure gold, but fireplaces can never be constructed of zinc or lead with safety to the dwellings in which they are built. So, too, the same measures which are efficacious in preventing the occurrence of intermittent fevers will not suffice to extinguish typhus, nor will those which extinguish typhus prevent the development and spread of enteric fever; and we might as well hope to prevent or to treat successfully all forms of metallic poisoning by a reference to one essential element from which all metals are differentiated, as to prevent or treat successfully all forms of fever by a reference to one essential cause of all fevers.

The theory of a change of type in disease is of necessity a theory closely connected with the doctrine of essentialism in



fever, because, of course, if all the various forms of fever are not essentially different forms of disease, but merely different manifestations of what is essentially the same disease, then, to account for these varieties, Stokes, and all who hold with him, are forced to maintain the obsolete doctrine of a change of type in disease. In speaking of this, Stokes says—

“ We can hardly conceive a revolution in practice more complete. In place of the loss of blood we have the exhibition of stimulants ; in place of a system of almost starvation we have the careful use of nutriment. This change of practice, depending on change in the vital character of disease, was followed by the charge against many of our predecessors and teachers that they were mistaken practitioners, ignorant of true pathology, and little better than blind followers of traditional error. Not only have their powers of observation been questioned, but even their morality and honour have been assailed ; and it has been suggested that the whole doctrine of change of type in disease was an invention to cloak former errors ” (p. 19).

But no one has ever brought a stronger charge against our predecessors than Stokes himself has done when he says, on the immediately preceding page, “ The doctrine of essentiality of disease was ridiculed ; venesection in fever was common, and *its traditional employment* was supported by modern theory ; the use of wine and other stimulants was forbidden, and *many a life was sacrificed to this unphilosophical method of looking at disease* ” (p. 18). The fiercest opponent of the doctrine of change of type in disease never made a stronger charge against our predecessors than has been done by Stokes himself in the passage we have italicised. If the reader will take the trouble to compare the two passages he will see that Stokes’ own charge against his predecessors and early comrades—for fifty years ago, and as he himself has most strikingly depicted (p. 17), bleeding was not a thing of the past—is an exact travesty of that which he puts into the mouth of a younger generation, barring the charge against their morality and honour, a charge which we indignantly disclaim ; this, however, is of the less consequence, as, if it has reference to any one, it refers to himself mainly as one of the chief offenders, because it is by him and those who agree with him that the doctrine of a change of type in disease has been in quite recent times revived.

It is a singular fact that among all the numerous correspondents who entered the lists of controversy in our pages immediately subsequent to the publication of the late Sir John Forbes’ admirable paper entitled ‘ Homœopathy, Allopathy, and Young Physic,’ in January, 1846, NOT ONE has made any men-

tion of any supposed change in the constitution of disease except Dr. Andrew Combe, who assigns it a very moderate share, indeed, of influence on practice. So that it is to Stokes, Christison, and their followers, that we owe the revival of this old and exploded theory, and it is for them to say why they have revived it. The theory of change of type in disease is a very old story, indeed, and has been hitherto always put forward to explain some difference in practice, differences which have prevailed in all ages, even for centuries before the publication of Galen's celebrated controversial epistle, "*De venæ sectione—adversus Erasistratum*," in the second century of our era. For it was in the immediately preceding century that Asclepiades made the remarkable statement that "*Apud Athenas et urbem Romam phlebotomia vexatos vel pejus acceptos esse pleuriticos; in Pario vero atque Hellesponto resumptos ac revelatos*." But he bled in other diseases freely enough; it was only the "*pleuritici*" who were unable to stand the loss of blood, while Soranus says that he found no difficulty in bleeding his "*pleuritici*" whenever he fell in with them, at Rome or anywhere else, "*nulla regionum discretione confusi*," and scoffs at the theory that other patients could bear a loss of blood which pleuritici could not;—so old a story is this dispute regarding type and treatment of disease.

In support of his views Dr. Stokes quotes with approval "*A Memoir*," by Sir Robert Christison, "on the changes which have taken place in the constitution of fever and acute inflammations in Edinburgh during the last forty years" ('*Ed. Med. Journal*,' January and July, 1858). Amongst the fevers there commented on Sir Robert has described as synocha a fever characterised by "*a truly sthenic, inflammatory, or excited state of the pulse*," which was the prevailing type in 1817-20, true typhus having been then very rare; and he gives a very vivid picture of the ardent heat of the skin, the hard, incompressible pulse, the florid hue of the venous blood, and the impetus with which it escaped almost *per saltum* from the vein, the vivid glow of the surface, and the distracting pain and pulsation of the heart and chest. But he has done more, for Sir Robert has recorded that this fever was characterised by a temperature "*ranging from 102° to 105°, at times even to 107°*," while the pulse varied from 120 to 160, and the replacement of this synocha and of its congener synochus in more modern days by typhus is evidently regarded by him as a crucial instance of change of type in fever. But mere substitution of one form of disease for another is not change of type; because smallpox prevails at one time, measles at another, and scarlatina at a third, no one would refer these alternations of disease to a mere change of its type,



yet the evidence that smallpox, measles, and scarlatina are essentially one disease is quite as strong as that relapsing fever, typhus, and typhoid are one disease, and smallpox in modern times has recurred epidemically quite as frequently as relapsing fever or synocha. The crucial test of change of type would be to show that synocha, when it did recur, had itself put on an asthenic type. From shortly after 1829 synocha quite disappeared, and did not return till 1843, and then with lineaments so distinct that Sir Robert had no difficulty in recognising it. He says—

“In the commencement of the epidemic I had been for some months off duty in turn as clinical professor, when I was called to see my colleague Dr. Bennett, who was convalescing from an attack of fever. . . . When he had detailed to me his case I told him he had sustained to all appearance an attack of my old friend synocha, whose face I had not seen for a good many years, that he was not yet done with it, and that he would have another three days’ attack, commencing on the fourteenth day. Dr. Bennett, surprised—I will not say incredulous—replied, that the relapse had no time to lose, as there were only three or four hours of the fourteenth day to run. It did, indeed, lose no time; for I must have scarcely reached home, from his house, a distance of one mile, before the rigor set in with violence” (*Ed. Med. Journ.*, January, 1858, p. 591).

Again, in January, 1870, synocha reappeared in Edinburgh, and from the account detailed by Dr. Claud Muirhead in the *Ed. Med. Journal* for July, 1870, we find that it presented no symptoms or signs in which it differed in any marked degree from the synocha of 1817. It is quite true that those who personally remembered that famous epidemic all declared that, as Christison had said of the epidemic of 1843, “the pulse was neither so frequent nor so strong; the heat was not so pungent; the glow of the integuments was less lively and less general” (p. 592). But when we come to contrast the actual facts of 1870 with those which have been recorded of the epidemic of 1817, we find that in 1870 in Edinburgh the pulse ranged from 120 to 130, while the temperature varied from  $102^{\circ}$  to  $104^{\circ}$ , and was occasionally over  $106^{\circ}$ , while, by careful observations made—often every two hours—in the London Fever Hospital, as well as by many accurate observers in Germany, the temperature during the last epidemic of relapsing fever was found occasionally to rise as high as  $108.7^{\circ}$ , which is a very high temperature indeed even for relapsing fever, far exceeding the ordinary limits of high temperature even in fatal disease, which, according to Wunderlich, only rarely exceeds  $106.7^{\circ}$  (Wunderlich, *New Syd. Soc. Translation*, p. 205). So that whatever value we may attach to the term “pungent heat” we are

perfectly warranted in stating that the sthenic character of the latest epidemic of synocha as measured by temperature in no respect fell short of that of the famous epidemic of 1817-20, and in making this statement, so far from impugning the accuracy of the observational powers of our predecessors, as Stokes says is too apt to be done, we rely upon them and appeal to them in proof of the accuracy of our views. A rapid, full-bounding pulse is characteristic of all epidemics of relapsing fever, not merely of that of 1817-20; and had the victims of the last epidemic been bled there is no reason to doubt that the blood would have been as florid and flowed with just as forcible a stream (almost *per saltum*) as formerly.

Fortunately for our patients, the present generation of physicians do not associate the idea "sthenic," whatever that may mean, with a bounding pulse and a high temperature, and do not of necessity feel themselves called upon to combat this idea by the detraction of blood. Dr. Welsh, the chronicler of the epidemic of 1817-20, who is the great advocate for the usefulness of bloodletting, bases his arguments upon the following facts: 1st, that in the Royal Infirmary of Edinburgh, from Jan. 1812 to Jan. 1817, the mortality from fever amounted to 1 in  $10\frac{1}{4}\frac{6}{9}$ ; 2nd, from Jan. 1817 to Jan. 1818 the deaths were 1 in  $15\frac{1}{3}\frac{6}{3}$ ; 3rd, from Jan. 1818 to Jan. 1819 the deaths were 1 in  $20\frac{5}{4}\frac{1}{1}$ ; but he adds, free venesection as a treatment in fever did not begin to be employed till towards the close of 1817, and did not come into general use till the spring of 1818, and to that he does not hesitate to ascribe the diminution in the mortality. There can be no doubt, however, that his facts are susceptible of quite another interpretation, and that, as Murchison has pointed out, the cases during the first period were mostly typhus, those during the last period mostly relapsing fever (p. 310 and p. 411). Be this as it may, however, a rigid analysis of his own facts gives no encouragement to the idea that venesection is a successful mode of treatment for relapsing fever, for of 364 bled from the arm, 20 died, or 1 in 18·2; of 189 only leeches, 10 died, or 1 in 18·9; while of 190 not bled at all, only 4 died, or 1 in 47·5. It is not correct to suppose, as Christison has hinted, that bloodletting was disused during the epidemic of 1843, because of its obviously asthenic character (p. 592); the reason was far otherwise. Cormack, in speaking of the apparent relief which seemed in some cases to follow bloodletting, says, "These beneficial changes," which even in this so-called asthenic epidemic still seemed to result from venesection, "were often not effects, though sequences, of the bleeding, as was satisfactorily proved by the very same changes frequently occurring as suddenly and unequivocally in patients in the same wards,



who were subjected to no treatment whatever." Or, as Sir William Jenner has observed, when speaking of a case of relapsing fever which had been bled in the London Fever Hospital, "Nature unaided by the loss of blood in many cases effected a much larger improvement in a much shorter space of time."

From an enlightened consideration of these reasons, therefore (*vide* 'Murchison,' pp. 43, 279, and 412), and not for any supposed change of type, bloodletting has been given up even in relapsing fever, the most sthenic form of continued fever that comes before us, the very latest epidemic of which has presented precisely the same sthenic characters—so far as these can be judged of by a comparison of the temperatures and states of the pulse recorded at the time, and apart from mere vague statements as to a *pungent* heat of skin, &c., which are insusceptible of proof—as it did in those palmy days of 1817—20, when bloodletting was regarded as the *sine quâ non* of successful treatment. Yet the mortality has not increased since 1847; it has seldom been over 4 per cent., and not seldom under 1 per cent. In 1858 Sir Robert Christison said: "My own convictions on the subject are so strong that I regard nothing as more likely than that, in the course of time, some one now present will see the day when a reflux in the constitution of fever will present it again in its sthenic dress, and again make the lancet its remedy." Little more than ten years after this the first part of this prophecy was fulfilled; fortunately we think for our patients, the last was not.

As the whole theory of change of type in fevers rests mainly upon the question whether relapsing fever is a distinct disease, or merely a more sthenic variety of continued fever of varying type; and, as we have shown, that so far as actual tangible facts are concerned, and apart from mere medical opinion, relapsing fever is, whenever it appears, and at whatever distant intervals, the same distinct fever, presenting the same distinct characteristics as it ever did, we may regard the question settled so far as change of type in fever is concerned. To pursue the subject further into the field of general disease as Stokes has done would lead us into tempting fields indeed, but fields which are far too extended for our present narrow limits; those who desire to pursue the subject further in this direction we may refer to the writings of Dr. Markham,<sup>1</sup> Dr. Hughes Bennett,<sup>2</sup> and Dr. George Balfour,<sup>3</sup> who have most conclusively shown the fallacy of this

<sup>1</sup> 'Bleeding and Change of Type in Disease.' London, 1866.

<sup>2</sup> 'The Restorative Treatment of Pneumonia,' Edinb. 1865. 'The Present State and Theory of Medicine,' Edinb. 1855, &c.

<sup>3</sup> "Hematophobia," 'Edinb. Med. Journ.,' Sept. 1858. "Cullen and Gregory

theory, and proved from the facts recorded in the MS. clinical lectures of Drs. Cullen and Gregory themselves, who practised medicine at a time when inflammatory diseases at least were of a truly sthenic character—if such a time ever existed—and when patients labouring under such affections, if they did not require, were, at all events, subjected to greater losses of blood than at any other period of medical history; that inflammatory diseases, and especially pneumonia, presented then precisely the same characteristics which it does now, in particular that its pulse was neither fuller, firmer, harder, nor more frequent than it now is, while it was not treated by large bloodlettings because of its sthenic character, but because of erroneous pathological views. While, therefore, we clearly acknowledge the rectitude of the motives of our predecessors, and marvel at the energetic boldness with which they carried out the treatment their pathology seemed to require, we agree with the last-named authors in seeing nothing in the past history of medicine to prove that either inflammatory disease or fever have been at any time more sthenic in character than now, or that they have ever required that excessive bloodletting by which they were formerly treated; and though the modes by which disease is produced and recovered from make it possible for it to be successfully treated in many various ways, especially by skilful physicians, we consider that the past history of medicine conclusively shows that the employment of perturbative medicine, and especially bloodletting, while it has been frequently productive of temporary relief, has never been free from very considerable danger—a danger which has been infinitely greater in some fevers than in others, for there are some, such as synocha, which may be therapeutically tampered with with much greater impunity than others, such as typhus.

Very closely connected with this part of our subject is the treatment of fever by cold affusion; to this Stokes only alludes in the most cursory manner, as a means of relieving the headache of fever of equal use with arteriotomy, at least, he says, in these degenerate days. But cold affusion is not merely a remedy for a symptom; it is long since it was propounded and practised as a cure for fever itself, and in recent times it is chiefly with this view that it has been employed. More than a century ago Lancisi wrote: “Nullum optimum ad curandos ab epidemicis febribus inventum esse remedium ipso nivis usu;” and about the end of last century (1787) Dr. Currie, of Liverpool, introduced cold affusion as a general treatment for fevers, and he



and his followers throughout the world claimed to have attained a very high degree of success. In recent times it has been employed by Hallman, Brand, Liebermeister, Jürgensen, and others, who have found that, though it does not cut short the disease, it abbreviates it, and reduces considerably the mortality of fevers, particularly enteric fever ; and it has been shown by Schroeder, of Dorpat,<sup>1</sup> and by Dr. Wilson Fox,<sup>2</sup> that the benefit obtained from cold affusion is due to the diminution of the hyperpyrexia and the consequent arrest of the excessive metamorphosis of tissue, upon the non-elimination of which many of the dangers of fever depend. Now, it is somewhat singular that the same authority which claims such a wonderful—thermometrically immeasurable—degree of hyperpyrexia for the synocha of 1817—20, should at the same time declare that this remedy, which past experience had shown to be of great service in fevers, and which modern experience and science proves to be of most use in those cases in which hyperpyrexia is greatest, was perfectly useless, if not worse than useless, during that epidemic. Christison says—"It gave relief for half an hour, but was of no permanent advantage, though often repeated;" and he adds that it frequently produced, after each application, such intense feeling of pressure and weighty pain in the head as could not but be regarded with uneasiness. This is a curious episode, which may obviously be interpreted in many various ways ; we only quote it at present as another instance of the varying character of medical opinion in relation to the action of remedies, and of the utter impossibility of putting any trust or confidence in it as a proof of any change in the constitution of any given disease, which, so far as it cannot be measured by physical agents, can command but a very limited and doubtful belief amongst all but the most credulous, and when it is in direct opposition to the facts revealed by these agents, cannot but be ignored by every one not blinded by prejudice, who is possessed of a *mens sana in corpore sano*.

Apart from the peculiar views upon which we have just commented, which are, however, so closely inwoven with all his sayings as to render Dr. Stokes' work a most unsafe one for a student, these lectures are extremely interesting and instructive, and had they been published thirty years ago would at once have taken a foremost place in medical literature. We find running through the whole work statements which show Stokes' firm belief in the essential and similar cause of typhoid, typhus,

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<sup>1</sup> 'Deutsches Archiv f. Klinische Medicin,' 1869. Bd. vi, p. 385.

<sup>2</sup> 'On the Treatment of Hyperpyrexia by the External Application of Cold.' London, 1871.

rheumatic fever, and even, it would seem (p. 153), of a contagious fever accompanied by the rapid development of miliary tubercles, a belief which, however opposed to modern pathology, is born of the native independence of the man, and based on the experience of half a century. During all these years he has seen the rise and fall of sundry schools of medicine and of various theories of therapeutics. He has seen the solidism of his early days replaced by the Humoralism of the Vienna school, and that again by the neuro-pathology of Henle, followed by the cell-pathology of Virchow, and that by the state of chaos as to all minute pathology in which we now are. He has witnessed the rise of auscultation and percussion as important means of diagnosis, and has himself most successfully aided in the development of these arts—a development which has progressed from the time when every disease was supposed to have its special acoustic sign, till now that these arts are solely regarded as means of ascertaining the exact physical condition of the parts investigated, a physical condition the cause of which we must take other means of ascertaining. He remembers the time when in the “old Meath Hospital there was hardly a morning that some twenty or thirty sufferers from acute local disease were not phlebotomised,” when the floor was so covered with blood that it was dangerous to attempt to cross it; when “the cerebral symptoms of typhus fever were met by opening the temporal artery, or by a large application of leeches to the head; and it sometimes happened that the patient died while the leeches were upon his temple—died surely, and almost suddenly,” so that an eminent Dublin apothecary assured him that during his apprenticeship there was hardly a week that he was not summoned to take off a large number of leeches from the dead body. He remembers the time when, from a terror of the effects of wine on the supposed inflammatory condition of fever, even good physicians ordered it with fear and trembling, and only in desperate cases one or two teaspoonfuls of light claret in cold water two or three times a day being considered quite a reasonable quantity; and it is to his own writings and example, as well as those of Alison and Graves, that a more liberal administration of stimulants has since prevailed—an administration which Todd and some others pushed at one time to an extreme as great and as irrational as the previous distrust had been, and it is in a considerable degree owing to a careful attention to the excellent criteria for the use of stimulants laid down by Stokes himself that more rational principles now prevail. And he has yet—while freely acknowledging the imperfection which prevails in every department of therapeutics—a wholesome distrust of the modern method of attempting to prove the action of



remedies upon diseased human beings by experimenting upon healthy animals. When we consider the varied phases of medical opinion and of medical practice through which Dr. Stokes has lived we are by no means disposed to quarrel with him for honestly preferring the views he has been able to ascertain for himself to those which are attempted to be thrust upon him by others. We know the fascination which surrounds the idea of the essential unity of the cause of all fevers, and are equally well aware of the difficulty in always clearly and distinctly differentiating the various forms of fever one from the other, upon which mainly the doctrine of unity is based; and thus, though we differ from Dr. Stokes in his estimate of the value of the evidence in favour of the specific difference of all forms of fever, we yet cordially acknowledge the very philosophic and instructive character of his lectures, particularly the last nine, which embody the experience of fifty years in the treatment of fever, and which cannot be read without exciting a feeling of admiration for the breadth and soundness of his views. Even the heterodoxy so conspicuous in the first part of Dr. Stokes' lectures does not prevent them from being full of instruction for those who can separate the wheat from the chaff; while, under all circumstances, they are important as recording the views of a practitioner of most wide and varied experience.

Dr. Budd's work on typhoid fever is likewise founded on the experience of a lifetime, but develops a view of the nature of this form of fever, which is essentially different both from that held by Dr. Stokes and also from that propounded by Dr. Murchison. For while Stokes holds, as we have seen, the doctrine that typhoid fever may arise from the same essential cause as typhus, and is but a variety of it; and Murchison, on the other hand, acknowledges the generic distinctness of the one form of fever from the other, acknowledges also the essential distinctness of their causes, but believes that both may be, and frequently are, generated *de novo*, typhus from the poisonous exhalations of overcrowded and squalid human beings, typhoid from that produced by the putrefaction of fæcal and possibly also other forms of animal matter; Budd has not only adopted and strongly enforced the doctrine that typhoid fever is a contagious, essentially distinct, and self-propagating form of fever, but he has done more, for he has also attempted to prove that it originates in no other way, accepting as correct the views which for more than thirty years have been taught by Professor von Gietl, of Munich, that whenever the poison of typhoid is contained in sewage it is invariably derived from the excreta of an individual already suffering from the disease, a drain being merely the vehicle for its propagation, or, in fact, "a direct

continuation of the diseased intestine." As an example of Dr. Budd's reasoning on this matter we quote the following :

"Let an isolated case of typhoid fever occur, and, merely because he cannot readily trace the disorder to its source, many a medical man will assert, without the slightest hesitation or doubt, that the disease has clearly originated spontaneously. Press him, and he will be ready at once with the argument that there is no reason why the conditions which *first* gave rise to it may not again give rise to it now. But let a young sinner with the peccant mark upon him swear ever so stoutly that the disease he bears has come on spontaneously ; let him argue as plausibly and as long as he will against the incredulity which refuses to see that the conditions which *first* gave rise to this disease may again give rise to it now, and the very same medical man will laugh him to scorn. In smallpox and syphilis, where from the nature of the facts the evidence bearing on this question is precise and sure, the answer is unequivocal. The history of these two diseases is at hand to show—as by a great standing experiment—that these specific contagions, so far from being things that spring up at every turn, have, in reality, come into being under conditions hidden in primeval darkness, and of which we have no experience. The history of many other contagious maladies is precisely to the same effect. The very significant fact, already mentioned, of the exclusive appearance of many among them, in the first instance, in particular geographical centres, where, sometimes only after the lapse of long ages, they have afterwards spread far and wide, leads to the same general conclusion." "We *know* (he adds) that the poisons of smallpox and of typhoid fever are multiplied in the human body as certainly as we know that corn is raised from its own grain. That these poisons are multiplied in any other way is not only pure hypothesis, but is an hypothesis that has besides the fatal defect of being gratuitous—of being in opposition to all analogy that is of any worth, and of being in one of these cases, at least, only tenable at all, by heaping one extravagant assumption on another."

The idea that there are certain geographical centres of epidemic poisons from which they only spread under certain favorable conditions has been amply illustrated in late years by the history of cholera, which in its own native clime has often been known under favorable conditions to originate at once over large districts of country, as in 1861, when in one night it broke out along the Ganges from Rajmahal to Mirzapore, a distance of six hundred miles, while in this country and climate it seems incapable of occurring spontaneously, and its progress from man to man can be traced by precisely similar agencies as those which propagate typhoid or enteric fever, contamination of the air or the water with the products of fæcal fermentation. In the case of cholera, however, there are few, if any, who



think of referring the disease to these products alone, but all trace it back to its own specific product or germ cell, which finds its way out of the body it has been bred in along with the alvine evacuations, and follows their course; and the general belief now-a-days is, that if we can keep cholera from our shores we need fear no spontaneous development of its poison here; and if by chance it should make good its landing, its progress may still be stayed by careful isolation of the sick, and disinfection of all those secretions by which it may be spread, and the history of past epidemics, especially the more recent of them, has proved that this view is correct. Now, the history of typhoid fever proves, we think, that there is a great probability in the view which regards its original focus as somewhere in Europe, and as the climate of Britain varies but little, especially at sundry times and places, from that of those parts of continental Europe where it has most prevailed, it seems quite a probable opinion that typhoid fever may occasionally originate here. Still, we know very well that, once originated, it is much more likely to spread by germs than by origination *de novo*, which can only be a possible, but not a common, occurrence, so that Budd's view is much more likely to be generally correct than Murchison's, and that all the more that from the latent manner in which typhoid fever frequently occurs, the length of time during which the germs may themselves remain inactive, till by varying circumstances they are set free to exert their baleful influence, as well as from the many insidious ways in which the contagion may be caught; it must always, in this country at least, be a considerably open question whether any individual case or series of cases have originated *de novo* or from contagion. For example, Dr. Murchison quotes as an instance of origination *de novo* the history of an outbreak of typhoid fever in 1865 in an isolated country house, 800 yards from the village of Ratho, and much further from any other house, in which there was no doubt that the drains and cesspool of the house in question were very much at fault, and the drinking water impregnated with sewage matter. Dr. Parkes, in reference to this case, has stated ('Pract. Hygiene,' 3rd ed., p. 73), on the authority of a relation of the owner of the house, that the fever was imported by a groom who came ill with it from Dundee. Dr. Sheriff, who has reported the outbreak, and who attended this groom, has, however, stated that he laboured under "a pure uncomplicated attack of acute bronchitis," and that several of the cases of fever had commenced before his arrival. But there is, we know, still another possible source of contagion in this case, for many years previously the village of Ratho had suffered under a lingering endemic outbreak of typhoid, chiefly

affecting the houses on the north and east of the village, past which a foul-smelling gutter ran, leading from an open privy half way up the village. It is not impossible that some of the workmen subsequently seized got their contagion from floating germs arising from this source. At all events, we know that a lady who was seized with typhoid in a large, airy, country house in the neighbourhood of this village in the summer following this local outbreak, and whose case was at first regarded as originating *de novo* from the putrid exhalations of a filthy cesspool, subsequently declared that shortly before the commencement of her illness, on stepping across the gutter referred to, she felt struck by a peculiar feeling of sickness and faintness, and was immediately impressed with the idea that she was about to become seriously ill. There seems nothing improbable in the idea that the workmen referred to, as well as this lady, were infected by germs or a contagium already formed, and not merely poisoned by putrefactive emanations. When we add to this the fact recorded by Von Gietl, that the germs of typhoid fever have been known to be latent in the middle of a dung heap for nine months, and then to have fatally affected one of two men who were removing it; when we know also that the propagation of typhoid fever has been effected by the medium of milk, as has been satisfactorily proved by W. Taylor, Ballard, Dr. Russell, of Glasgow, and more recently in the Marylebone epidemic by Dr. Murchison himself; we can see the extreme difficulty of ascertaining, with anything like certainty, whether any given case has originated afresh or merely from contagion. That typhoid fever is contagious, and mainly by means of the patient's excreta, there are but few who will now-a-days have any doubt, notwithstanding that in several manuals of the practice of medicine this idea is spoken of as an illusive hypothesis. Even Murchison, who believes in its self-origination, believes in the contagious properties also fully; and if there be any who still have a lingering hesitation to accept this doctrine, we cannot do better than recommend them carefully to study the admirable work of Budd. This work has been written with a purpose. Proceeding on the idea that typhoid fever never originates, but is only propagated, and recognising the power which various chemical agencies possess of destroying the infective power of the intestinal discharges, he urges that the spread of this fever may, by the exercise of ordinary care, be quite prevented, and the disease itself be finally extinguished. It will certainly be a long time before we attain this desirable consummation; but the prevention of the spread of the disease is certainly very much within our reach. There is no reason, however, why we should ignore Murchison's pythogenic theory, though we believe in



Dr. Budd's; for many reasons, besides the possibility of the origination of typhoid fever, it is desirable to keep our houses and our drinking-water free from contamination with sewage, either in its gaseous or liquid form. But there can be no reasonable doubt that disinfecting the excreta of every typhoid patient would be a most powerful method of arresting this fell disease, which twice within the last twenty years has cast its shadow on the throne itself, which at this moment is filling scores of English homes with all those agonising uncertainties with which recent experience has made the nation familiar, and which during the last month have been consummated by the death of the patient in hundreds, and, if we extend our view to Europe generally, in thousands of cases.

Dr. Budd concludes his work by a most pathetic appeal to his fellow-countrymen to set the seal of sincerity on the public thanksgiving to God in which they were so recently engaged by sparing from this hour no human effort to eradicate this pest; and he warns them that though they may evade this duty, they can never evade the penalties of its neglect; "and he that was never yet connected with his poorer neighbour by deeds of charity or love may one day find, when it is too late, that he is connected with him by a bond which may bring them both at once to a common grave."

## Bibliographical Record.

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**Gordon on the Gold Coast.**<sup>1</sup>—Dr. Gordon's position when serving on the Gold Coast in 1847—48 led him to accumulate a series of notes on the climate and mortality of the district, which are as true now in 1874 as they were at the time when they were primarily penned. For Africa is a land which changes little, and the recitals of Barbot, Bosman, and Snelgrave are now as true and as vivid representations of daily life as those of Burton, Baker, or Schweinfurth. Dr. Gordon's convenient little volume gives a number of useful facts, a few of which we shall cull for the benefit of future travellers to the Gold Coast, or conquerors of Ashantee. That there have been young men on the Gold Coast, as in all other parts of the world, who have by their own habits induced disease and death is a fact beyond denial. It is no less true, however, that the mortality thus caused is but a very small item, indeed, in that which prevails among our countrymen there, and that, in reality, the abstemious and temperate are by no means seldom the first to succumb. When, in 1824 and 1826, white troops of the old African corps served on this part of the Coast, circumstances in regard to them were very different from what they now are. The men were desperate and reckless characters, the knowledge of hygiene possessed by medical officers was not acted upon to the extent it now is, and, as a result, we learn that the average duration of the life of each man upon the Coast was less than one month. It would be wrong to anticipate so sad a result at the present day; yet, bearing in mind that the physical conditions upon which much of the sickness really depend are the same in 1873 as they were half a century ago, it is right to acknowledge that sanitation has, indeed, a heavy task before it.

It would appear by published statements that in future floating hospitals are to be stationed off the Coast for the reception of sick, and it has been proposed to establish sanatoria at various places, as Fernando Po, Ascension, &c., to which patients may be sent for recovery. Dr. Gordon's experience on the Coast leads to the belief

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<sup>1</sup> *Life on the Gold Coast.* By CHARLES ALEXANDER GORDON, M.D., C.B., Deputy Surgeon-General. London, 1874,



that, although floating hospitals have assuredly many advantages, neither they, nor sanatoria established at the places indicated, can be looked upon as substitutes for the immediate and direct removal to the United Kingdom of all men who suffer to any considerable degree from climatorial disease. Frequently the surf at Cape Coast runs so high that communication with the shipping is difficult, or even impracticable. It therefore seems very necessary to have sufficient accommodation on shore for all who require it, and it so happens that ample space exists a little to the westward of the Castle for the erection of huts and all necessary buildings for the purpose. But, in fact, nothing short of immediate removal home of men who fall sick will be sufficient for their recovery, for assuredly a man who once is prostrated by endemic disease will be of no further use during the present expedition. Only those who have themselves been ill on the Coast can understand the intense craving for home felt by them. The sense of being unable to return acts as a most powerful depressant, whilst the prospect of return has an equally stimulating and beneficial effect. Similar results have been observed elsewhere. For example, during the late China war a sanitarium was established at the Cape of Good Hope, and thither some of the sick were sent from Hong Kong. These men, however, did not value the change; they felt as much abroad there as at Hong Kong, and, therefore, if they were to die, one foreign place was no better than another. Such is an example of what sick soldiers actually feel and express. On the Gold Coast all these feelings will be intensified, and hence Dr. Gordon believes the necessity of providing amply in this country for sick soldiers and for sick officers also, and despatching both classes without delay to England. In dealing with tropical fever it is obvious that the author attaches more weight to a depletive rather than a recuperative treatment. This opinion is the reverse of that of most visitors to tropical climates, who are persuaded that tropical fevers, whether in Africa, India, or Central America, are really associated with a condition of system in which the strongest tonics are not only necessary, but advisable. To apply the strict precepts of temperance writers to localities far apart from England appears to us to be injudicious. The true secret of the management of large bodies of soldiers or employés is not to forbid the use of alcohol, but to regulate its quantity, and ensure its perfect quality and pureness, and to moderate its use.

The general tenor of life at Cape Coast Castle appears certainly to have been in the time of Dr. Gordon rather convivial, if we can judge from some verses he quotes, written by the acting judicial assessor.

Notwithstanding all the drawbacks to residence on the Gold Coast, we see a healthy British population able to maintain their military position often at fearful odds against hostile tribes. It would be difficult for any individual, without personal knowledge of

the natives on the West Coast, to realise the all but complete absence of interest on the part of the people in all that surrounds them. In fact, they seem not to bestow even a passing thought upon the phenomena that take place before their very eyes. It is true that their calculations of time are made in accordance with the increase and wane of the moon; they believe, however, that on each succeeding month an altogether new satellite is produced, although how, does not appear. The sun, according to their belief, performs a daily journey round the earth; stars in their courses watch over individuals for good or evil, and in respect to all other branches of knowledge their ideas are equally crude. Their knowledge of astronomy seems to begin and end with observing that, as they express themselves, "It is midnight; the southern cross begins to bend." We trust that, as we are still to hold possession of the Gold Coast, that the next generation which reads of the efforts of Gordon and Wolseley and others, to preserve life in one of the most pestilential spots of the old world, will meet with more success than has as yet been granted.

**Works on Diseases of the Ear.**<sup>1</sup>—1. As a history of the progress of knowledge in the department of surgery of which it treats, probably no more satisfactory book has been published in English than Dr. Roosa's; and the same may be said of the account which that same author gives of aural surgery as at present practised in Germany, England, and America. The treatise gives evidence, not only of the most exhaustive reading on the subject in hand, but of very considerable personal experience of the practice of the leading aurists in Europe.

The first chapter consists of a brief outline of all that has been written on the anatomy and surgery of the ear from the time of Hippocrates up to 1869. The book is divided into four parts, which treat of the external, middle, and internal ear, and to this is added a good account of deaf-mutism.

The descriptive anatomy of each division of the ear embraces the researches recently made by German anatomists, and a similarly complete account is given of all the plans of treatment (including

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<sup>1</sup> 1. *A Practical Treatise on the Diseases of the Ear, including the Anatomy of the Organ.* By D. B. ST. JOHN ROOSA, M.A., M.D., Professor of Diseases of the Eye and Ear in the University of the City of New York.

2. *Traité Théorique et Pratique des Maladies de l'Oreille et des Organes de l'Audition.* Par le Docteur J. P. BONNAFONT. Deuxième édition, revue et augmentée.

3. *The Questions of Aural Surgery.* By JAMES HINTON, Aural Surgeon to Guy's Hospital.

4. *Lectures on Aural Catarrh; or, the Commonest Forms of Deafness and their Cure.* By PETER ALLEN, M.D., F.R.C.S. Edin., Aural Surgeon to St. Mary's Hospital.



operative proceedings) which have from time to time been recommended by those who are in the front rank of German and English surgeons, viz. Gruber, Politzer, Weber, Tröltzsch, and Hinton. The chapter on foreign bodies in the external meatus is especially deserving of notice, and it is pleasing to find that, when he comes to treat of those affections which form so large a proportion of the cases which come under the notice of aural surgeons, viz. diseases of the middle ear, Dr. Roosa has arrived at very decided views on the pathology of these affections. He divides them, as is now usual with most noteworthy writers, into non-suppurative and suppurative, but of the former of these two divisions he distinguishes two classes, the catarrhal and proliferous forms of inflammation. Of the catarrhal, Dr. Roosa says (p. 263)—

“I think we may assume, from the history of cases, that this form of disease is either a consequent of frequent attacks of acute catarrh of the middle ear, or that it occurs in people who have what we may call a catarrhal diathesis. The disease, therefore, unlike its companion, proliferous inflammation, is not at all insidious in its approach. The patient suffering from this disease, who consults us about his hearing, is usually aware there is an excess of secretion in his pharynx,” &c.

Again (p. 268)—

“If we now turn to the picture of the subjective symptoms of what I term proliferous inflammation, we shall find them much less positive than those of the catarrhal form. Some of the patients have no subjective symptoms at all, except that of the loss of hearing, which is, of course, an objective symptom as well. They have no sore throat, no increase of the secretion of the pharynx or nostrils. . . . The origin of this form of aural trouble cannot be traced back to infantile earaches, frequent coryzas, or to naso-pharyngeal catarrh. It is a peculiarly insidious affection, one which is usually under full headway, and which essentially impairs the function of hearing long before the patient is aware that he has any affection of the ears.”

In the first of these forms the pathological appearances are, according to Dr. Roosa—

1. Collections of mucus distending the cavity of the tympanum.
2. Thickened mucous membrane.
3. Filling up the cavity by lymph.

In the second—1. Connective-tissue formations in the cavity of the tympanum. 2. The mucous membrane of the tube covered by dense fibrous tissue. 3. Hypertrophy of the bony walls of the tube. Hypertrophies of bone, exostoses, and anchyloses in the tympanic cavity. It is needless to add that, in the more serious form of disease of the middle ear, treatment is not likely to prove of such value as it does in the catarrhal variety.

Weber's operation of division of the tensor tympani; Politzer's and Lucae's of incision of the posterior folds of the tympanic membrane; Hinton's incision of the membrane for the removal of mucus, are each carefully described, and the views of the author on the propriety of these modes of treatment are given as follows:

"I. Paracentesis, or incision of the drum-membrane in chronic non-suppurative inflammation, is by no means a dangerous or painful procedure.

"II. Its chief value is in furnishing a means of treating the lining of the middle ear.

"III. It may properly be performed in cases of chronic proliferous inflammation, that are still advancing in spite of local treatment, through the Eustachian tube.

"IV. Division of the tendon of the tensor tympani and division of the adhesions existing between the membrana tympani and the walls of the cavity of the tympanum, are operations that deserve a trial in cases of chronic inflammation of the middle ear, with symptoms of increased auricular pressure, not relieved by a fair use of the ordinary means."

From the obviously extended practice of Dr. Roosa it would have been gratifying to find mention of more personal experience in connection with operations for diseased conditions of the middle ear; but enough has been said to show that no one greatly interested in the subject of aural surgery can afford to omit the reading of a treatise, on the general merits of which there cannot be two opinions.

2. From the long established position of M. Bonnafont as an aural surgeon, another edition of his work on diseases of the ear almost demands perusal by those who take an interest in the rapidly advancing knowledge in this class of diseases; and, furthermore, the book may, we presume, be considered as at least one of the principal representatives in literature of French surgery in connection with the ear. Without further preface, it may be said at once that, comparing it with the recent exhaustive handling of the subject by Drs. Joseph Gruber and Adam Politzer, of Vienna, it falls very far short, both as a "theoretical and practical treatise."

There are certain methods of examination and treatment which, when once they are made common property, so manifestly commend themselves for simplicity and good effects, that they receive almost universal adoption by earnest workers. Such are Dr. von Tröltsch's method of illumination for the examination of the tympanic membrane, and Politzer's method for inflation for the middle ear. In the work before us the first is not referred to, and the second has received but scanty recognition.

To those who are interested in the early literature of aural surgery the references in the first few chapters will be found to be full of



information; and if half the space that is in this way employed were taken up with some account of what has been lately written in Germany and this country, M. Bonnafont would have provided a treatise more serviceable as a guide for reference in cases of difficulty. On the subject of foreign bodies in the external auditory meatus a number of cases are cited, which, besides being interesting and instructive, show that the author is possessed of a very large experience; but on this subject there are a number of elaborate instruments described and figured, the utility of which seems questionable where more simple measures are at command, and, certainly, in the hands of the inexperienced these instruments might prove the reverse of useful. For harmlessness, ingenuity, and simplicity, the plan proposed by Dr. Lowenberg, in a short paper published some time ago, is worth the whole chapter. It cannot be too often urged that in the case of a foreign body in the external auditory meatus, excepting when a stream of light is reflected, and so the whole passage illumined, no attempts at removal beyond syringing should be made, as the offending body is harmless when compared with the injury which so frequently follows the introduction of instruments when unattended by effective illumination.

M. Bonnafont attaches more importance than is usually accorded (at least since Mr. Toynbee's time) to the condition of the tympanic membrane as producing deafness, the precise cause for this (except in the instances of marked bulging in parts, relaxation, and perforations) being now generally estimated as due to the morbid conditions in the cavity of the tympanum.

Throughout the book the cases are well reported, and are evidently selected from a very large number at hand, so they will be found useful for reference. This, and the almost elaborate accounts of the writings of those who have preceded M. Bonnafont, tend to make the treatise, perhaps, of greater literary than practical value.

3. In 'The Questions of Aural Surgery' Mr. Hinton gives the results of his experience in the treatment of diseases of the ear, in language so well chosen that, apart from the value which the book possesses for those working at the subject, it will compare very favorably with the medical literature of the day. From the beginning to the end the author is addressing the earnest workers of aural surgery, and he presupposes a certain degree of knowledge in his readers. This he states in the preface, as follows:

"I have felt it the less necessary to go into every detail of the subject, because of the recent publication of other works, of which such details must be a repetition; as, for example, the translation I have had the pleasure of making of Dr. von Tröltsch, 'On the Surgical Affections of the Ear,' and Professor Helmholtz's 'Treatise on the Mechanism of the Ossicula,' for the Sydenham Society, and Mr.

Dalby's 'Lectures on Diseases of the Ear,' which give an excellent outline of the subject. I may refer also to a very complete work, easily accessible in this country—Dr. St. John Roosa's 'Treatise on Diseases of the Ear.' "

Throughout the book, in relating cases, reference is made to a series of beautifully executed water-colour drawings (lately published), which represent the morbid conditions of the tympanic membrane.

It would occupy too much space to do full justice to all that Mr. Hinton has to say; but the following points which are touched upon are, perhaps, among the most noteworthy. All degrees of change from health in the appearances presented by the tympanic membrane are carefully noticed. Anomalies in lustre and curvature, thickening, bulging, thinning, appearances of bubbles seen through the membrane, discolorations, both of which latter shift with the movements of the patient's head, and are due to secretion in the tympanic cavity; collapse of the membrane, bladdery protrusions, chalky deposits, and perforations. In long-standing catarrh the thinnings of the membrane are observed (he says) in the posterior and superior part, and are due to the pressure exerted by masses of dense secretion :

"I have certainly seen more bulgings of the membrane from collections of mucus in this part than in any other, and it seems to me that it is due to its entanglement in that position owing to the presence of the chain of ossicles; and we cannot but feel how probable it is that mucus that had collected in this position, becoming dense and dry, should give rise to increasing deafness after all appearances connected with increased secretion had passed away."

The statements of other observers, that the conduction of sound is interfered with less by changes which have taken place in the membrane than in the cavity behind the membrane is verified as follows :

"In states of perfect hearing the membrane may present considerable varieties of appearance, differing greatly in hue, in transparency, and sometimes presenting even dense masses of chalky deposit, and occupying a considerable portion of its surface. In one instance of not only perfect hearing, but acute musical sensibility, in a child, there existed a solid rod of bone, running from the short process of the malleus to the posterior wall of the tympanum, apparently a malformation of the bone."

Again—

"Cretaceous deposits are quite frequent, even in healthy membranes; they may coexist with perfect and even with acute hearing;" and "It would seem that the peculiar structure of the membrane has less to do with the transmission of vibrations than it might be natural to suppose."

For the treatment of the simple cases which so frequently come under the notice of an aural surgeon, Mr. Hinton recommends those plans which are most generally adopted. Thus, varying degrees of



obstruction of the Eustachian tube are treated with Politzer's inflation, or the air-douche through the catheter, and astringent solutions to the faucial orifices of the tubes; and when the tympanum shares the condition of the tube, fluid solutions injected through the catheter; when the secretion from the lining membrane is in excess, a weak solution of the sulphate of zinc; and when the dry stage has been reached, alkaline solutions. When, however, obstruction of the tubes is not overcome by ordinary means, he is in the habit of using laminaria bougies through the catheter, and in his hands this treatment has been followed by very good results. The practice of making an incision into the tympanic membrane for the better evacuation of mucus retained in the tympanic cavity has been in other places<sup>1</sup> fully discussed, and so we find a brief repetition of the indications for the operation, with the additions which an enlarged experience has suggested.

"A most exaggerated feeling," he says, "prevails respecting the danger of making incisions in the membrane, which is curious when we consider how freely the cornea is operated upon. It is difficult to keep the cut open for so long as four days."

That distinct benefit is often gained in appropriate cases of catarrh of the middle ear by operative proceedings on the tympanic membrane has of late years been abundantly proved on the authority of the chief aurists of the day—Gruber, Politzer, Schwartze, Tröltzsch, and others; so that Mr. Hinton in the above sentence only confirms the experience of others.

On the structure of aural polypi the reader is referred to the account given by Mr. Dalby of these growths, and the complete destruction of the surface from which they spring is urgently insisted upon. To ensure this, the frequently repeated application of chloro-acetic acid is recommended after the polypus has been removed, and but scant mention is made of any other plan of treatment, and this obviously, as he has found no occasion for other methods, his own succeeding so well.

On the subject of nervous affections of the auditory apparatus there are many cases reported, which are of great clinical interest; but in the way of treatment for these disorders no new light has been thrown, so far as we can gather from the author's remarks; and he confirms the observations of those who met at the last conference of aural surgeons at Dresden, as to the small value of electricity as a remedial agent.

In conclusion, we may congratulate Mr. Hinton upon having, in his own words, made it "plainly appear that, so far from disease of the ear being a barren or unattractive field, it is one full of promise."

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<sup>1</sup> 'Guy's Hospital Reports.'

4. The first edition of Mr. Allen's book appeared in 1871, and was noticed at some length in the October number (1872) of this Review. The chief additions to the second edition are two illustrations (one of Brunton's ear speculum, and another of the mode of using the Eustachian catheter), the introduction of the somewhat wordy term "psophometer," applied to the piece of india-rubber tube which connects the ear of the patient and surgeon in examination of the Eustachian tubes and tympanum, and a more complete discussion of Mr. Hinton's operation of incision of the tympanic membrane. With reference to this latter, the author, in the first edition, confined himself to remarking that it "had been extensively, and in some instances very rashly, adopted in London," and that he himself "had witnessed cases in which its evil results had been conspicuous, and permanent deafness with difficulty averted."

In the second edition the propriety of making an incision into the tympanic membrane in cases of accumulation of mucus in the adjoining cavity is more frankly dealt with, but the subject is finally dismissed as follows :

"After very careful and repeated perusal of Mr. Hinton's reports of successful cases of incision of the membrana tympani, as well as the unsuccessful one related by him,<sup>1</sup> I am firmly convinced *the whole of them* could have been equally benefited, and the tympanum cleared of the accumulations in other ways than by incision."

Enviably as may be the power of arriving at an opinion without the necessity of examining the patient, with our at present limited knowledge, we shall prefer an account by the surgeon who has seen the cases to that of one who has not had such opportunities.

Happily, just now, our best medical literature is noticeable by the absence of anything approaching the sensational; but we here find, in the instances of relieving obstruction of the Eustachian tube, "The expression of joy by some patients that have been successfully operated upon is almost rapturous; a new world is, as it were, being opened to them by the instantaneous restoration of hearing." A similar "world," we presume, is opened by many surgical proceedings which give relief, such as the restoration of sight, the emptying of a distended bladder, and so on; but it is not usual to describe these effects in such glowing language—at least, such a style of writing will commend itself to the public rather than to those of the profession who seek for information.

**Proctor's Practical Pharmacy.**<sup>2</sup>—Practical pharmacy is treated of in this volume in a novel and somewhat attractive style. Twenty-

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<sup>1</sup> 'Guy's Hospital Reports, 1869.'

<sup>2</sup> *Lectures on Practical Pharmacy.* By BARNARD S. PROCTOR. London, 1873.



nine lectures are given, and the author has of purpose preserved much of the colloquial style of the lecture-room. To understand the circumstances under which the book saw the light, it is necessary to read Mr. Proctor's preparatory letter prefixed to the volume, giving an account of the establishment of a Lectureship on Practical Pharmacy, in the Newcastle College of Medicine, and the appointment of the author as lecturer. These printed lectures are, indeed, mainly a reprint of Mr. Proctor's professorial lectures.

The plan of the work is this. A lecture is given, occupying about a dozen octavo pages, illustrated, when necessary, by excellent woodcuts; and this is followed by a short series of questions for self-examination, and a recapitulation of the leading points of the lecture. The book is thus admirably adapted for self-tuition. The student can read a lecture, making such experiments as are described, and with the assistance of the Pharmacopœia, he can then test his knowledge by writing out answers to the questions for self-examination; and he can correct these by a reference to the "recapitulation," which is, in fact, a summary of what the answers to the questions ought to be. The whole of the operations of practical pharmacy are carefully explained, and many valuable criticisms are passed upon the value of some of our official and other preparations.

We will give a few instances of these latter observations:

"*Decoct. Aloes Compositum*.—This is one of the most important decoctions in the Pharmacopœia. . . . It is a common trade custom to prepare decoction of aloes four times the pharmacopœial strength, it being stated that 1 part of the concentrated decoction mixed with 3 of water makes the decoction of the British Pharmacopœia. This, however, cannot possibly be the case, as the quantity of tincture of cardamoms would have to be 32 ounces, while the whole product would only be 30" (pp. 163, 164).

"Wine of iron has nothing to recommend it but the taste. It is not very active, is very indefinite, and not tempting in appearance. The wine of the citrate has at least the advantage of greater uniformity. Aromatic iron mixture may be said to have all the advantages of the vinum ferri without its pleasant taste" (p. 384).

"The granular effervescing preparations known as citrate of iron, citrate of iron and quinine, carbonate of iron, and iodide of iron, form a little family. It were better they had never been born. Their birth showed but the folly of their parents, and their decay shows that they did not possess the stamina for a useful existence" (p. 385).

Not the least valuable lecture is that devoted to the reading of autograph prescriptions. It is illustrated with thirty-two full-page fac-simile lithographs of actual prescriptions, all of which present some points of difficulty. It is impossible, without a reproduction

of the lithographs, to do justice to this chapter of the work. We will merely remark that the *principles* which should guide a dispenser in interpreting a doubtful prescription are admirably laid down, and, if carefully studied in the manner Mr. Proctor suggests, will prove of essential service to the tyro.

We have read this volume with much pleasure and profit. It is one which should be in the hands of every pharmaceutical chemist, and the physician and general practitioner cannot fail to profit by its perusal. The fault of Mr. Proctor's book is diffuseness; but this is, perhaps, hardly to be avoided without losing the colloquial style of a lecture.

**Swain's Surgical Emergencies.**<sup>1</sup>—This Manual “is intended to supply what the author believes is a wide-spread want, viz. a small book containing directions for the immediate treatment of all those various emergencies with which the general practitioner may be called upon to deal at any moment.” Such is the *raison d'être* which the author assigns for his work. So far as the main part of the book is concerned, namely, that which relates to surgery, it may well be doubted whether such a manual as this was in any way needed. There are several small text-books and works on minor surgery which cover this part of the field, and do it more fully and more thoroughly than our author. But the peculiar feature of this work is, that it includes not merely surgical emergencies, but those also which belong rather to medicine, *e. g.* emergencies arising in parturition and in cases of poisoning. Looking at its entire scope, we doubt whether there is any book which is designed to occupy the whole of this ground; and we may, therefore, believe that it will be useful, that it will help some of our professional brethren in sudden exigencies, and thus contribute to lessen human suffering and to prolong human life.

That the work will be well done we have sufficient guarantees at the outset. Mr. Swain is well known as an able and skilful medical practitioner, who has had large experience in operative surgery, and in those emergencies with which he here undertakes to deal. He has, moreover, called to his aid some able coadjutors; and additional value is given to his little book by the fact mentioned in the preface, namely, that Mr. George Lawson has revised the chapter upon “Injuries to the Eye,” and Dr. Alfred Meadows that upon “Emergencies connected with Parturition.”

In a work of this kind, when the author professes only to deal with a limited portion of a large subject, he is met by the difficulty

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<sup>1</sup> *Surgical Emergencies; together with the Emergencies attendant on Parturition, and the Treatment of Poisoning; a Manual for the use of General Practitioners.* By WILLIAM PAUL SWAIN, F.R.C.S., Surgeon to the Royal Albert Hospital, Devonport. London, 1874.



of deciding what he is and what he is not to include, and he lays himself open to criticism both for faults of omission and of commission. For example, in this Manual, in speaking of Colles's fracture of the radius, nothing is said about the ordinary methods of treatment by the pistol splint or by the straight splint, but a long account is given of "Gordon's splint," and this is the only one which is mentioned in connection with the subject. Now, Gordon's splint, no doubt, is a very good one, but our subject is "Surgical Emergencies," and in an emergency it is highly improbable that Gordon's splint would be at hand. Fancy a general practitioner called five miles from home to see a case of urgent accident. He finds that it is a fracture of the lower end of the radius. He turns up the subject in Swain's Manual, and the only method of treatment mentioned is by a rather complicated splint, which perhaps he has never heard of, and which he certainly has not got in his gig! We do not for a moment object to the value which is set upon Gordon's splint, but surely other and simpler methods of treatment should at least be mentioned, if they are not explained in detail.

Again, the preface tells us that "the chapter on antiseptic treatment has been kindly written by Dr. Bishop at the request of Professor Lister, and embodies the most recent and exact directions for the effectual carrying out of this method." This chapter we have studied with great interest. Mr. Lister's method of dressing wounds is of so much importance to the practical surgeon, that we were glad to read an account of it which was brought up to the latest details, and, as it was given in a work upon surgical emergencies, we hoped that it might be presented in such a form as to adapt itself to the exigencies of general practice. But we were sadly mistaken. Unless there is some more rough-and-ready way of applying Mr. Lister's principles, his method is certainly not at all suited to the requirements of general practitioners. Dr. Bishop's account of it is very clear and lucid, but it leaves us with the conviction that it is far too complicated for use in surgical emergencies.

Take only one point in the system as it is here set forth. Mr. Lister, as we all know, attaches much importance in certain cases to the creation of an antiseptic atmosphere around the wound. How is this to be produced? Only one method is here mentioned, and it is the following: "This is provided in the form of a spray of 1 to 40 carbolic acid solution, for which the most easily managed and efficient instrument will probably be found to be a steam spray apparatus, manufactured for Mr. Lister by Mr. J. Gardner, 45, South Bridge, Edinburgh." So, when a general practitioner is called to a case which he thinks would be best treated by the antiseptic method, he must send off to Edinburgh for a *steam spray*

*apparatus!* We believe Mr. Lister's method has been found impracticable in military surgery, on account of all the minute details which are essential to its success; and for the same reason we believe it will be found unsuited to surgical emergencies. If in such cases the surgeon wishes to employ antiseptic dressings, he must be contented to apply them in some much simpler way than that recommended in this Manual on the authority of the distinguished Edinburgh professor.

Again, surely any work which undertakes to assist general practitioners in emergencies, and which departs so far from the strict lines of surgery as to include emergencies arising from parturition or from poisoning, ought to contain some allusion to the immediate treatment of fits—apoplectic and epileptic fits—which are among the commonest emergencies that general practitioners are called to. On the other hand, it seems hardly necessary in a work of this kind to allude to rupture of the vas deferens, a very rare accident, and one which is not attended by any urgent symptoms. Indeed, so rare is this accident, that it is not even mentioned in the latest editions of Fergusson's, Erichsen's, or Pirrie's well-known works on surgery.

We mention these points, and we might mention others of the same kind, to show how difficult it is to deal with a restricted subject such as Mr. Swain has selected. If he goes over old ground he may well be asked to show cause why his book was published at all. If he omits the well-established dicta of the schools, and recommends only novel methods of treatment, he may place the practitioner who relies upon him for guidance in a very awkward position.

A book for emergencies should deal in a clear, brief, dogmatic way with what is most certainly known with regard to the cases in hand, and the methods of treatment recommended should be as simple as is consistent with efficiency. When an emergency arises it is not the time to try new contrivances or to discuss the merits of novel methods of treatment. We must say that, on the whole, our author has borne this in mind, and acted upon it so far as the narrow limits of his space permitted. Some subjects are remarkably well handled, *e.g.* retention of urine and strangulated hernia. In many instances, also, we have noticed that valuable practical hints are given which we do not remember to have seen elsewhere, and which no doubt the author has learnt by personal observation at the bedside.

On the whole, the perusal of this little book has left with us a high opinion of Mr. Swain's judgment and resources as a surgeon, though we are scarcely satisfied of the need there was for the work, or of the author's wisdom in entering upon it.



**Prosser James on Laryngoscope.**<sup>1</sup>—This little volume will be useful to the student who is desirous of learning the action of the laryngoscope, and the purposes to which it may be applied. It is essentially elementary, more so than its rather comprehensive title would imply; and the portion which deals with therapeutics and treatment is scarcely such as we had a right to expect from one who boasts to have been the first to resort to laryngoscopic medication.

It is certainly surprising that no really good and comprehensive work on the larynx and its treatment has as yet appeared in England—a work which should deal intelligibly with the anatomy of the parts in health and disease, and give simple directions for treatment without elaborate descriptions of obsolete instruments, or of processes which have long been abandoned.

The *brochures* of Morell McKenzie, admirable as they are, the writings of Sir D. Gibb, of Dr. Geo. Johnson, of Mr. Norton, and others, can scarcely be considered an adequate outcome of all the laryngoscopic work which English medical men have accomplished.

Without receiving any marked development, the study of throat diseases has been much extended during the last few years, and the united experience of the many workers in this field would be invaluable if digested and condensed into a handy volume, well illustrated with plates prepared for the purpose.

In Dr. Prosser James' work the coloured plates form a novel and very useful feature. We cannot, however, look without surprise on one which is held to illustrate "a most satisfactory instance of arrested consumption" (Plate III, fig. 2). The arrangement of the volume seems to have been pretty much a matter of chance; the chapters appear to have sorted themselves. It will be found, however, that many points of interest are discussed in its pages, and if there is nothing absolutely new, there are many facts which will bear repeating, and many hints which the tyro in laryngoscopy will do well to keep in mind. We are glad to see that Dr. James enters a protest against the rude treatment of the epiglottis proposed by by some German and French writers; for there is no doubt that a little patience and quiet determination will overcome all difficulties, and render "even the insertion of the left forefinger of the physician to support the valve" an unnecessary proceeding. Chapter VI would have been better if it had given an anatomical description of the muscles, nerves, and ligaments of the larynx, instead of limiting the sketch to the reflexion seen in the mirror; and a few typical cases, such as are seen in every-day practice, of hoarseness and aphonia, might have been added with advantage to the very general sketch which is given of topical medication. This little

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<sup>1</sup> *Lessons in Laryngoscopy, including Rhinoscopy and the Diagnosis and Treatment of Diseases of the Throat.* By PROSSER JAMES, M.D. London, 1873, pp. 176.

work, however, contains much general information, and with a more orderly arrangement of subject, and such additions as we have ventured to suggest, it might worthily fill a niche among school manuals, and do its part in popularising a very special and important branch of medical study.

**Silver's Practical Medicine.**<sup>1</sup>—Happily for the credit of the profession, and for the well being of the public, the increased demands made of students by the examining bodies, and the improved examinations, have driven the meagre manuals of past years—compiled for candidates for this or that diploma, and guaranteed as safe guides to get them through the equally meagre examinations—out of the market. Nevertheless, a demand exists, and will ever exist, for concise treatises on the different departments of medical science, and it is one which our younger physicians and surgeons are always ready to supply. A reference to the publishers' catalogues might, indeed, suggest that, at the present time, the requirements of students are sufficiently met. Manuals and handbooks meet our eyes in every bookshop, each aiming to make as royal a road as possible to its special subject, by removing difficulties and elucidating obscurities, with every possible aid from typography and engraving. But still new volumes issue from the press, many of the handbooks astonishing us by their bulk, and proving, we apprehend, alarming to students who want rudimentary and not exhaustive treatises. The fact is, writers of manuals strive to include in their volumes every known fact and every leading opinion; and, as a necessary consequence, build up a considerable monument of their industry and research, which, like too concentrated food, proves indigestible to the learner, who needs feeding with the milk of science, and not with the strong meat.

We suspect that the publishers of the treatise before us must have entertained some such opinions as those now expressed, and wished to bring out a book on medicine which could be rationally designated a manual. With the able assistance of Dr. Silver they have succeeded in this object sufficiently well, although we are not disposed to value the groundwork whereon the book is based. Indeed, we should have preferred a 'Sketch of Practical Medicine' to have come from Dr. Silver's hands unfettered by any trammels imposed by the attempt to rehabilitate 'Meade's Manual.' This last was really a "grinder's" book, intended to smooth the way of those too idle or too stupid to prepare themselves legitimately to pass their examinations and to duly qualify themselves for their profession; and, in our opinion, it should have been allowed to have lapsed into

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<sup>1</sup> *Practical Medicine, with a Sketch of Pathology and Therapeutics; being the Fourth Edition of 'Meade's Manual for Students.'* By ALEXANDER SILVER, M.D., &c. London, 1874.



oblivion. Indeed, we regret Dr. Silver mixed up his name and reputation with Meade. By so doing he appears in less desirable company, and fails to get credit for the vast amount of good work he has done, by assuming the position of a mere reviser of a manual which could make pretence to no other character than that of a "cram." We comprehend well enough that the whole thing is a publisher's scheme to utilise an old property—a copyright; but we are sorry that Dr. Silver fell in with the scheme. Nevertheless, the buyers of the book have good cause to congratulate themselves on the excellence of its contents generally, for we except from its merits the anatomical, physiological, and the so-called therapeutical portions. They are of the Meade stamp, and next to worthless to every genuine student of the subjects they deal with. The part devoted to "Therapeutics" is, in fact, simply a catalogue of drugs arranged according to their supposed action in disease, with a few remarks on their virtues, and a notice of their doses.

In this judgment of the contents of the book we feel we are in accord with Dr. Silver himself, who, in the preface, tells us that the work must be judged "as a book on the practice of medicine," and as one primarily intended for students. From these aspects we therefore willingly judge it, and the result of our examination is to satisfy ourselves that it presents a concise outline of the practice of medicine well suited to students who wish to peruse in a condensed form what they have imbibed by lectures and have read in works of more complete and exhaustive character.

We have examined its contents at large to form an opinion relative to its teachings and to the manner in which knowledge is imparted, and our conviction is, that Dr. Silver has succeeded in conveying the main facts and the accepted doctrines of the day in pathology and therapeutics in a correct and intelligible form; though, indeed, an improvement might be made in many places in the composition, and it would be possible to point out inaccuracies and omissions, and to show cause for disagreement respecting some details of treatment. But the decided merit of the condensed outline of the practice of medicine Dr. Silver has produced, and his own expressed recognition of defects and imperfection in design and execution, disarm the critic and cause him to review with very favorable notice the production as a whole.

**Dickson on Mental Medicine.**<sup>1</sup>—A melancholy interest attaches to this volume. It has been bequeathed to us, and should be accepted as the last utterances of an accomplished physician and

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<sup>1</sup> *The Science and Practice of Medicine in Relation to Mind, the Pathology of Nerve Centres, and the Jurisprudence of Insanity; being a Course of Lectures, delivered in Guy's Hospital.* By J. THOMPSON DICKSON, M.A., M.B. Cantab. London, 1874.

teacher, cut off in the prime, though not the vigour of youth, who had done good work as a psychologist, scientific experimentalist, and artistic illustrator, who had secured an ample share of fame in his own specialities, and afforded a bright promise of a distinguished and useful future.

Dr. Dickson was engaged in correcting and extending these lectures, delivered in Guy's Hospital in 1869 and 1873, when suddenly, but not unexpectedly, he died of disease of the heart in his carriage. His present production is in some respects crude, even rash; but these qualities are not the result of ignorance, but of great vigor, originality, and, perhaps, prematurity of experience; but, whatever their ultimate value may prove to be, they should be received as the fruits of personal observation and conviction. The lectures were, moreover, delivered clinically and amid the pressure of practice, so that the topics discussed cannot fail to bear the stamp of being regulated rather by collateral circumstances than by any system of classification.

We differ widely from the opinion of Dr. Dickson, that a knowledge of the constitution and powers of the healthy mind is altogether needless or superfluous as introductory to the study of insanity, and that all requisite information as to the functions of the brain may be obtained in the observation of its diseases, seeing that precisely the same arguments apply to all the truths revealed by physiology and to all organs of the body. Notwithstanding this view the author adopts a psychical basis for classification, preferring to every other the time-honoured, but, what is of more importance, the time-tested division of morbid mental states into amentia or original privation, dementia or acquired deprivation, mania or excitation, melancholia or depression with enfeeblement; all of these being complicated by delusion. The prevailing tendency towards a pathological arrangement is, in our opinion, in the present fluctuating it may be progressive, state of our knowledge of structural and functional changes, premature; and, seeing that, such nosology must be commensurate with all known diseases, and must keep pace with every discovery as to the nature and relations of such diseases is absurd. Dr. Dickson gave much time and gained much reputation in endeavours to establish a connection between the different forms of derangement and pathological appearances in the brain and nervous tissues, visible or microscopically visible, and was most successful in perpetuating, by means of preparations, the results of his observations. He regards imperfect or vitiated nutrition as the foundation, directly or indirectly, of all cerebral affections, and we have many valuable suggestions as to brain-wasting, brain-exhaustion, and cell-impairment, although Dr. Dickson writes rather vaguely of progressive changes in mania, as follows:

“It is, however, only in cases where the defect has never ex-



ceeded atony that the apparently perfect recovery, after a number of years of mania, can take place; for if the brain is structurally changed, the cells which during the first stage of the disease lost their faculty for comparison and correction, cannot possibly return to their ordinary and healthy activity."

He admits very emphatically that the same disease leaves more palpable traces in

"Formative changes, indicative that a low subacute form of inflammation has existed, by which the membranes have become thickened, and the areolar tissue, which, with the vessels, tacks the pia mater to the brain, is increased. The thickening of the dura mater is sometimes enormous. I have collected specimens the thickness of ordinary chamois leather. Sometimes the skull is enormously thickened. The diploe has entirely disappeared, and the whole thickness of the bone is a dense mass of sclerotic bone-tissue," &c.

In the second place, it should be noted that great importance is evidently attached to the connection of post-mortem appearances with particular forms and stages of mental disturbance, and that there is an obvious disposition to identify some of the more minute and recently detected alterations with particular phases of alienation. He notes, (*a*) the observation by Dr. Lockhart Clarke, of holes and tubules in sections of the white substance of the convolutions and of the optic thalami, regarded as widened perivascular spaces with brain wasting; (*b*) the diminution of the grey substance and the disappearance of the caudate cells, another cause of brain loss, the laminae noticed being looked on as widened perivascular canals, because the walls of the cavities are perforated by the smaller channels which convey branch vessels, whilst some of them exhibit, on examination, the sheaths of the vessels which remain behind, although the vessels themselves may have become destroyed or absorbed; (*c*) the appearance, independently of increase in the quantity of fluids in the ventricles and on the surface of the brain as compensating for its diminished volume, of granulations of the ependyma, and particularly on the surface of the septum lucidum and fourth ventricle, supposed to be hypertrophy of the epithelium, and as indicative of an acute inflammatory condition. In all this the author boldly pronounces against Dr. Blandford's thesis, that the cause of delirium and death in acute and rapid cases of insanity, in delirium tremens, and the like, is stasis and embolism of the capillary circulation, the result of pressure, or inflammatory change in the blood. And he most honestly and truthfully confesses that—1, notwithstanding the advances we have lately made in brain physiology, it will be long before we can isolate any set of cells, and say from definite appearances, "these are the cells of madness;" 2, that we cannot point to a brain, and say with absolute certainty,

from the conditions presented, "that is the brain of insanity," and that the microscope has not demonstrated one special alteration in the brain which can always be recognised as the cause of subjective phenomena; 3, that brains are constantly met with presenting every feature of morbid change, those of wasting included, where no melancholia nor corresponding phenomena have been detected during life.

In connection with his favourite views as to innutrition and inanition, and what constitutes a most important feature in the treatment of the insane, compulsory alimentation, Dr. Dickson mentions two important practical facts—1. That whenever the ribbon-weavers at Coventry were thrown out of work, there were always numerous applications from that district for admission for patients with acute mania, in a starving condition, into St. Luke's Hospital; 2, that, in experiments conducted by Professor Haughton, where the waste of substance was calculated by placing the fasting human body in a heated room, it became necessary to abandon the test in consequence of the mind almost giving way under the protracted abstinence.

Dr. Dickson, although what may be called an energetic practitioner, shrunk, we regret to see, from the employment of opium in mania, from some fanciful idea that it checked the secretions and carbonized the blood, altogether disregarding the practical considerations that it economized strength, that it produced rest and repose; that it mitigated pain; that it suggested pleasing thoughts and emotions; and that, according to the experience of some authorities, it acted as a "strait-waistcoat upon the cells," or, curatively, as a check upon formative changes. He is as sceptical as to the benefits attributed to digitalis, although he has given it to the extent of half an ounce; and to those of henbane; but in both instances admits that the uncertain strength of the drug may be connected with the failures which he records. He is somewhat more favorable to *Cannabis indica*, in mania complicated with epilepsy or menorrhagia, but places the greatest reliance upon chloral, alcohol, and abundant food. We must guard against any supposition that Dr. Dickson has withdrawn his allegiance from narcotics upon the ground that they are antiquated or altogether effete, or for any other reason than that his own experience has not tallied with that of his predecessors; for he, with a courage and independence which do him infinite credit, and in defiance of popular clamour and crotchets, has given in his adhesion to certain forms of restraint, seclusion, darkness, to the influence of a strong mind over a weak one, and other old-fashioned measures, as valuable adjuncts in the treatment of mania.

The treatise will be found very useful as a student's textbook, and as a repertory of recent and typical cases to the ordinary practitioner.



**Legal Responsibility in Old Age.**<sup>1</sup>—This is rather a preface or programme to a large work in preparation upon Longevity and the correspondence between age, or, perhaps, it would be more correct to say, between bodily and mental development, than a treatise in itself. The title is likewise a misnomer, as a very large portion of the inquiry is occupied in demonstrating that the manifestations of genius, and, indeed, of intellect and intelligence generally, correspond with development, and we are allowed but few glimpses of the wide statistics upon which the conclusions of the author are affirmed to be founded, although many of these are open to discussion, if not to doubt, notwithstanding the emphatic and rather ambitious language in which they are propounded. The author advances, as the general results of his investigation, the statement that “the golden decade is between 30 and 40, the silver decade between 40 and 50, the brazen between 20 and 30, the iron between 50 and 60, the tin between 60 and 70, the wooded between 70 and 80.”

We are not disposed to dispute this rather arbitrary arrangement, nor the proposition that the greatest mental efforts, the most important and useful discoveries and achievements, have been made between 30 and 45. But when we find that the best test of development or the best dynamometer for testing the growth of the mind is found in our capacity for appreciating one of Shakespeare's plays—“As the mind grows, its power of appreciating Shakespeare increases, and the great advantage of this particular test is that the mind cannot overgrow it”—we must demur to a crux which is solely applicable to highly imaginative or highly cultivated minds. Any general standard would involve a fallacy if applied to measure the height and breadth of the capacity of men of different race, temperament, and culture. It is true that Dr. Beard has confined his analysis to prominent and representative men; but because in the small number of examples selected the most brilliant and profound thoughts originated in a particular decade, it would be an egregious error to infer that the average amount of psychical power was then attained, that maturation and even elimination of creative powers then ceased in all men, and that a sharp line of demarcation could be drawn between the results of genius and of experience.

Dr. Beard formalises the belief of the great majority of his countrymen in saying that the brain follows the same line of growth, maturity, and decay, as the rest of the body; that the nervous, muscular, and osseous systems rise, remain, and fall together; and that the received opinion that the mind, of which the brain is an organ, develops and matures later than the power of motion or of physical labour and endurance, is not sustained by the facts of

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<sup>1</sup> *Legal Responsibility in Old Age; based on Researches into the Relations of Age to Work.* By GEORGE M. BEARD, A.M., M.D. (Read before the Medico-Legal Society of the City of New York, March, 1873.) New York, 1874.

history. His corollary from this and other arguments, that Judges, and, we would add, all those subjected to much brain work, should be enabled to retire from active duties at 70, and we would stipulate much earlier, is one in which all reflective men, on both sides of the Atlantic, would cordially concur. But, although there is truth in the intellect being the eye of conscience, and that when it is blinded by disease or the decay of age, men cannot distinguish the true path, even though they desire to do so, we cannot admit the principle which Dr. Beard's researches appear to involve, that in age itself, apart from all other conditions and considerations, there is a tendency to moral degeneracy and decrepitude. We cannot enter upon the list of illustrations given; many of the individuals named can still answer at the bar of the world as to their perception of right and wrong, sound sense and folly; but as to the detailed cases of moral perversion in advanced years, the same symptoms, the same monstrous and inexplicable changes in character have been observed at all ages, and even in the purest and noblest members of society.

While a monograph on the legal responsibility of old age still remains unwritten, we can recommend the present introduction to the subject as displaying much bold and independent and acute thought, and many eloquent passages.

We smiled, and only smiled, over this passage:—"In England the Juries, although they do badly enough, yet seem to agree better than in this country. I account for this by the fact there is less average intelligence in England"—and wondered whether a play of Shakespeare could not be employed as a test of capacity under such circumstances.

**Psychology of Scepticism.**<sup>1</sup>—This work is the product of a thoughtful, but not of a clear-thinking mind. The author has dwelt much with the metaphysicians and psychologists—more, perhaps, in the Borderland which connects their respective domains, contracting somewhat of mysticism from the one side, and somewhat of materialism from the other, but failing, we suspect, so far as his inferences have been apprehended by us, to satisfy either of these parties, and certainly to carry conviction to independent and logical thinkers. His great object is expressed in his last sentence, "to show, in general, how phenomena are possible, and with the demonstration of their possibility, Scepticism and Phenomenalism, as systems, disappear." In the attempt to demonstrate this and "that of an external substantial world outside of us we have no reliable proof;" he enters largely into the errors of Locke, in making this proof depend upon impressions on the senses; into the philosophy of Berkeley, which

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<sup>1</sup> *The Psychology of Scepticism and Phenomenalism.* By JAMES ANDREWS. Glasgow, 1874.



dissipated these errors by showing that we have no consciousness of impressions, but solely of ideas, and that this cannot be separated from a "perceiving spiritual substance;" and into the destructive analysis of Hume, which elicited that the very arguments employed by Berkeley against the trustworthiness of sensations are applicable to his own theory of ideas, "as these consist of many perceptions which had no connection among themselves; they must, therefore, inhere to this unknown spirit." When arrived at this region of the unknown, this annihilation of the evidence of both matter and mind, the author seems to vacillate long between doubt and the creed that all consciousness, or acts of what we call mind, acquire a fixity of tenure by their demonstrable connection with nervous matter; but, at page 21, incredulity reappears in the sentence, "It is not the sceptic that deludes me, dreaming or waking; delusion is the normal result of healthy mental action; it does not necessarily imply disease, corruption, or misguiding of our faculties." It can, we think, be anatomically and physiologically established, that the simplest as well as the most complicated condition, whether it appears to proceed from within or from without, whether it indicates an internal or external substratum, or anything beyond itself; whether it merely reaches and builds up, or actually constitutes, the Ego, is in some manner dependent upon "as a function of or an addition to" some of the materials subservient to life. But this nexus appears either to leave the arguments of the philosophers untouched, or to add new factors of fallacy to the whole investigation, as it shows that the perception of a colour, whether that indicate extension, and, therefore, a material origin or not, must depend upon the health or unhealth, the activity or atony, and a thousand other states of the tissues with which the perception itself is associated. Much of the darkness, and consequently discussion, which have arisen from this subject may be traced to the total forgetfulness of V. Cousins' views on the Me and the Not Me, and of the older doctrine of Relativity, by which the existence of one thing is proved by the existence of another, by the similarity and dissimilarity of their qualities.

Mr. Andrews, aware of this, devotes much time to the consideration of Hartley's theory of Association, and endeavours to point out its defects by insisting upon the distinct and independent function of each nerve, forgetting that the sense of touch, to which he so constantly refers in illustration, is conjoined in the same nerve with those of pain, heat, resistance, and so forth. He endeavours to solve one of the difficulties connected with this point by the untenable proposition—"the localisation of objects which otherwise would be known like joy, without being localised, the association depends on a particular distribution of nerves to each special part, which distribution being interrupted, the association abruptly ceases."

We look upon his theories as to the use of what he calls the

“organ of consciousness,” as to the “unconscious memory of the nerves of touch,” and numerous other points which bristle on the surface, as temptingly courting question, but such excursus is foreign to our present purpose; and we can only recommend the pamphlet as an interesting specimen of the problems which are engaging the attention of our hard-headed, brain-working countrymen of the North.

**Progressive Muscular Atrophy.**<sup>1</sup>—The attention of pathologists has recently been much directed to the subject of this monograph, and more particularly to the histology both of the muscles and of the nervous centres. Our journals and the transactions of medical societies abound with clinical reports of cases, while to many of the fatal cases the result of a microscopical examination is appended. A history is incomplete without its histology. Dr. Friedreich has here given the details of seventeen cases, with a description of the changes observed after death in the muscles, nerves, and nervous centres, in both true and false hypertrophy and muscular atrophy. The author has further levied contributions from almost every writer who has treated of, or mentioned the matter in hand. He begins with a list of twenty-three references to authorities, whilst every page is loaded with references and foot-notes, which “bristle” with figures and data.

The book positively defies a short analysis, and must be itself consulted by all who would know what has been done, up to its date, to elucidate the pathology of disease in the muscular system. The indefatigable industry in literature manifested by his countrymen has long since become proverbial, but the author before us has outdone ordinary German research. We must therefore content ourselves with referring our readers to Dr. Friedreich’s treatise.

**Folie Héréditaire.**<sup>2</sup>—We have here three lectures on one of the determining conditions of mental derangement. The influence of hereditary predisposition is disputed by few, and those are compelled to admit, in its place, a family tendency, which it would be difficult to dissociate from the condition of heredity. Hereditary insanity is treated at considerable length by Griesinger, whose lecture on this topic might have served as the basis of the lectures before us. In these latter, however, the evidence is treated more in detail. The phases in which it presents itself, its varying degrees, and its allied constitutional degeneracies, are all fully expounded. In the majority of treatises

<sup>1</sup> *Über Progressive Muskelatrophie, über Wahre und Falsche Muskelhypertrophie.* Von Dr. N. FRIEDREICH, Professor der Medicin in Heidelberg. 4to, with two plates. Berlin, 1873.

<sup>2</sup> *La Folie Héréditaire (Hereditary Insanity).* By Dr. LEGRAND DE SAULLE. Pp. 74. Paris, 1873.



upon mental disease the question of its hereditary character is dismissed in a few pages or paragraphs. In Dr. Legrand's lectures the subject has been completely considered in all its bearings; they may, therefore, be commended to the attentive consideration of our readers.

**Le Délire des Persecutions.**<sup>1</sup>—The interest attaching to this work is mainly derived from the fact that it contains the results of the author's observations, made during the turmoil, the trials, and the afflictions consequent upon the last siege of Paris. As physician of Bicêtre and of the Prefectural Dépôt, Dr. Legrand has seen in the chiefs and partisans of the insurrection, and in the hostages of the Commune, the influence of political excitement in the causation of disorder of reason, the perversion of conscience, and the defection of the will. Amidst events so moving as to have plunged France into mourning, and catastrophes so lamentable as fell *coup sur coup* upon Paris, Dr. Legrand has not failed to find a scientific interest in the cerebral and psychical conditions of a population thus heavily tried.

Dr. Legrand, as one inference from his observations, regards it to be an erroneous opinion that political events exercise a marked influence on the development of insanity, raising the number of its victims, and leading largely to unlooked-for cerebral maladies. Revolutions and riots, Dr. Legrand remarks, do but disturb the minds of the predisposed to mental disorder, and accelerate the end that might have been independently foreseen. Any other exciting cause beside the overthrow of a throne, or a street fusillade, would have produced the same result. Great social perturbations do not produce disastrous consequences upon the intellect of a whole nation, since they are but temporary in their action. From 1847 to 1854 a uniform number of lunatics were admitted into the Bicêtre, due regard being had to the fluctuations of the Parisian population during that period. Out of a thousand lunatics, M. Morel found only five, from 1848 to 1856, whose mental derangement could be traced to political events.

"To impress upon the human brain," Dr. Legrand adds, "a pathological blot, there is required a slow, continued, and progressive change in the education and public habits and manners. There must be prolonged excitement of the passions and excessive expenditure of cerebral activity or of vices. It is under the influence of these disturbing causes that the functions of the nervous system become perverted, that insanity breaks out, and suicides increase in number."

The increase of insanity in France during the last thirty years

<sup>1</sup> *Le Délire des Persecutions.* Par le Dr. LEGRAND DE SAULLE. 8vo, pp. 524. Paris, 1873.

must be attributed to relaxed or vicious education, to a total absence of faith, want of morality in literature, to self-indulgence, thirst for gold, reckless commercial speculation, sudden reverses of fortune, and to increase of alcoholism and debauchery. Dr. Legrand, however, gives its share in the mental derangements of the period to the events of the siege, and describes the pathological features of the several stages of the catastrophe as developed in acute melancholy, suicides, epilepsy, melancholy with stupor (*stupidité*), and the delirium of persecution. It would be impossible, in our space, to give an approach to the details of these transactions and their influences, but we submit that the whole tenor of Dr. Legrand's work is to testify to the indirect influence of political disturbances in augmenting the number of victims of at least one form of insanity.

The delirium of persecution, however, is, after all, but a form of melancholia—a *species*, as it is designated by Dr. Legrand, who asks if he is not justified in according an exceptional importance to a form of mental alienation which attacks, on an average, 500 persons per annum in Paris.

The principal characteristics of the lunacy of persecution will readily be recognised as those of melancholia. The patient hears insulting or accusing voices, which warn him of plots and dangers, or urge him to avoid them by suicide. The most insignificant circumstances are misinterpreted by him; a passing smile is an intentional expression of contempt; the sound of wind conveys threats; the sound of the clock transmits injuries; whispers betoken conspiracies and machinations; the thunder is to him an electrical proof of coalitions formed against him, &c. He declares himself the subject of malignant influences—physical, magnetic, electrical; that he is the object of secret poisoning, and so forth, the ordinary features of the melancholic lunatic.

Dr. Legrand's work, we should add, contains a large body of facts and copious narratives of cases of the "persecution mania." As a contribution to the literature of mental alienation it will be welcomed by all who are specially interested in that department of pathology.

**Liveing on Leprosy.**<sup>1</sup>—We are glad to see Dr. Liveing's 'Gulstonian Lectures' revised and enlarged, and published in a separate form; and we think the little volume will take and keep a respectable place among the numerous works relating to the interesting disease of which it treats; for, though it does not contain very much that is not to be found elsewhere, it gives us a good deal of trust-

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<sup>1</sup> *Elephantiasis Græcorum, or True Leprosy.* By ROBERT LIVEING, A.M. and M.D. Cantab., F.R.C.P. London; Physician to the Middlesex Hospital. The 'Gulstonian Lectures for 1873,' revised and enlarged. London, 1873. 8vo, pp. 150.



worthy information in a compact and readable form. Dr. Liveing tells us in his preface that he has had some special opportunities of observing the external circumstances under which the disease is developed in Norway, where (contrary to what has taken place in other European countries) it appears to have been on the increase during the past forty years (p. 39). In his first chapter he gives a slight but interesting account of the disease in Europe during the Middle Ages, with a very brief notice of Jewish leprosy; and on this part of the subject we will offer a few remarks chiefly corroborative of the view which he takes, and supplementary to his observations.

We must here notice the almost entire absence of references to the authorities for the statements contained in the work, which we cannot but consider a defect, as in several passages we should certainly have liked to consult the original authorities if the opportunity had been given us. For instance, in the remarks on the nomenclature of leprosy and elephantiasis (p. 2, &c.), we think it would have been better to have quoted or referred to the passages confirming his explanation (which we believe to be substantially correct), and it seems to us to be worth while to attempt to supply the omission. The Latin word "*lepra*" is used to signify both the *leprosy* of the middle ages and also the *lepra vulgaris* of modern nosologists. We cannot explain *how* it came to be used in two such different senses, but we think we can trace the confusion to its fountain head; at least we will put together, at the risk of some degree of tediousness and minuteness, a few points, which (so far as we are aware) have hitherto escaped observation, and which appear to us to be not unworthy of the attention of the medical antiquarian and historian. The treatise which appears among the works of Constantinus Africanus (vol. i), under the title "*De Morborum Cognitione et Curatione*," is known to be a translation of an Arabic work, by Ibnu-l-Jezzár, who lived in the tenth century. This work also exists in Greek and in Hebrew, and therefore enjoys the honour (unique probably among the older medical writings) of being found in no less than *four* different languages.<sup>1</sup> In this work the chapter on *جذام judhám*, or *true leprosy*, is headed *Ἐλεφαντίασις* in the Greek MS.,<sup>2</sup> and "*De Elephantiasi*" in Constantine's printed Latin version (p. 160); but in the body of the chapter the disease is called *lepra*, which is probably the earliest instance of the word being applied to *ἐλεφαντίασις*.<sup>3</sup> Why he should

<sup>1</sup> Perhaps the best account of this singular literary curiosity is to be found in the late Dr. Daremberg's '*Notices et Extraits des Manuscrits Médicaux, &c.*,' p. 63.

<sup>2</sup> For the information relating to the MSS. in the Bodleian Library at Oxford we are indebted to the Rev. H. O. Coxe and Dr. Neubauer.

<sup>3</sup> As an additional proof that this chapter relates not to *λέπρα*, but to *ἐλεφαν-*

have used two different Latin words, and not synonymous, to designate the same disease, we cannot tell; but it is probable that this chapter of his work has been one of the chief causes of the confusion that has prevailed as to the nomenclature of the disease ever since. The next chapter is on *برص* *baras*, which is rendered *λέπρα* by the Greek translator, and *morphea* by Constantine. In the same way his treatise entitled "De communibus medico cognitu necessariis locis" corresponds very nearly with part of the great work of Haly Abbas, who lived in the tenth century; and here, too, the chapters on *جذام* *judhám*, and *برص* *baras* (*Theor.*, lib. viii, capp. 15, 16), are headed by Constantine "*De lepra*" and "*De morphea*" (vol. ii, p. 228), but by the Latin translator of Haly Abbas, more correctly, "*De elephantia*" and "*De lepra*."

It is not improbable that Constantine may be also partly answerable for the confusion as to the Latin word "*elephantiasis*;" at any rate we have seen that he uses this word in one passage to signify true leprosy, while in another<sup>1</sup> he uses it in the sense of *داء الفيل* *dá-l-fil*, or *Barbadoes leg.* In both cases we simply state that Constantine, who lived in the eleventh century, is the earliest writer that uses the words in the more modern sense, *who has yet come before our notice*, but we do not venture to say that no earlier authority may yet be found.

In the fourth chapter, on the clinical history, morbid anatomy, &c., of leprosy, Dr. Liveing mentions the complication of this disease with *scabies*, which is "so commonly met with in Norway, and which is interesting, because some observers have believed that it served as a means of propagating the disease by contagion" (p. 138). It is also interesting as illustrating, if not explaining, the singular statement of Justin ('Hist.,' lib. xxxvi, cap. 2), that the Israelites were driven out of Egypt because they were subject to "*scabies et vitiligo*," representing respectively the Greek words *ψώρα καὶ λέπρα*. These two diseases are not unfrequently mentioned together by the ancient medical authors, and these are the very terms used by Josephus when confuting the same tradition ('Cont. Apion.,' lib. i, cap. 34). It is not necessary to suppose that *ψώρα* signified only the precise disease that now bears the name "*scabies*;" but if we believe it to have been sometimes complicated with the (so-called) *leprosy* of the Jews (whatever that disease may have been), it will help to explain in some degree the origin of the curious tradition mentioned above, and especially its propagation

*τίσις*, it may be mentioned that the writer refers in it to a passage in Galen concerning this latter disease, "De Simplic. Medic. Temper. ac Facult.," lib. xi, tom. xii, pp. 312, 313.

<sup>1</sup> 'Commun. Loc.,' lib. viii, cap. 18, vol. ii, pp. 230, 231, answering to Haly Abbas, 'Theor.,' lib. viii, cap. 18.



by the enemies and revilers of the Jewish nation. It will also render more intelligible the belief that the Jewish leprosy was occasionally contagious; for, though the subject admits of no positive proof, yet it is difficult to imagine that all the rites and ceremonies described so minutely in Leviticus (xiii and xiv) related only to ceremonial pollution, and had no reference to the danger of contagion.

We had intended to have noticed some other passages in Dr. Liveing's little volume, but our remarks have already run on to a greater length than we anticipated, and therefore we will only again express our generally favorable opinion of the work.

**Longevity.**<sup>1</sup>—Dr. Gardner commences his book by the consideration of the question as to the natural or normal duration of human life, and he agrees with Flourens in fixing the period at from ninety to one hundred and five years. He then states that the purpose of his work is, not to trace human life from birth to old age, to point out the causes which cut it short, or to indicate the circumstances which tend to preserve it, but only to treat of one of its epochs, namely, "advanced age," or the "decline of life," and, in his own words, "to show what is the precise nature of the changes in the constitution which take place in that epoch, and constitute in the aggregate *ageing*, to point out the antidotes and means furnished by science and experience for ameliorating or avoiding those troubles, and for retarding the effects of time, and thus for prolonging life." (p. 7). He then argues that the duration of life is partly a matter within our own power, and he points to many well-known circumstances and conditions which seem in the present day to conduce to longevity, such as absence of care, sanitary improvements, provident and temperate habits, and the progress of the healing arts, medicine and surgery. After some general remarks on the physiology of advanced age, Dr. Gardner indicates the means he recommends for ameliorating and retarding the effects which age induces; but there is little need for specifying his directions, for they are such as any well-informed medical man would give if his advice were requested. Dr. Gardner evidently disbelieves the theory of the total abstinents, for he recommends the moderate use of wine and other fermented liquors as a necessary portion of the diet of advanced life, and he is also clearly favorable to the use of tobacco. As an habitual drink he recommends beer, either porter or ale, taken with the food, but not exceeding eight ounces in quantity, and if these fluids are replaced by wine, especial regard should be had to its quality; and if, as he says, "the pure blood of the grape can be obtained, the selection of the kind to be preferred is of less moment." He is, probably, right in stating that the light wines of France are suitable

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<sup>1</sup> *Longevity; the Means of Prolonging Life after Middle Age.* By JOHN GARDNER, M.D. Pp. 168. London, 1874.

for six months of the year only in our climate, and that for the rest of the year good port is to be preferred. Of sherry he seems to entertain no favorable opinion, as he thinks it too often sophisticated, but he says that Marsala is "much recommended." In this part of his book, as indeed in almost all the others, the observations and directions are offered in a very loose and unsatisfactory manner. Beyond the mere statements, for instance, that port is a good wine for half an English year, that sherries are often sophisticated, and that Marsala is much recommended as "likely to be pure," no information whatever is given as to the preference to be shown for one wine above another, or as to the peculiarities of constitution for which each may be especially suitable. He only tells us that "the number and names of wines submitted to our choice is legion, and it should be a rule to avoid for ever any wine found uncongenial to the stomach or to produce headache."

On the whole, it is difficult to form a just opinion of Dr. Gardner's book. Some of his previous publications have been meritoriously directed to the dissemination of sound views in several departments of general and medical knowledge, and his present work contains some useful remarks and precepts on the arts of preserving health and prolonging life. But, on the other hand, if it is offered for the perusal of the general public (as perhaps the author intends it), it is too technical, and if for the guidance of the medical profession, it is too superficial, and indeed, in many particulars, quite unsatisfactory.

The directions to aged people who are passing too much water, and feel themselves growing rapidly weak, to test their own water for albumen (?), and, if they find any, to put themselves on a skimmed-milk diet (called by Dr. Gardner the "milk-cure," but we believe erroneously, the word being *molkencur*), as an almost infallible remedy, are surely calculated to do as much harm as good; and the caution that *dysuria*—which Dr. Gardner defines as *a simple overflow of water* (?)—may, if neglected, lead to Bright's disease, diabetes, oxaluria, &c., is too briefly worded to be intelligible. The directions given for preventing stone in the bladder are equally brief and equally obscure. If the urine is acid, then abandon acid wines (but, says Dr. Gardner, immediately afterwards, "all wines are more or less acid"), lessen the proportion of animal food, &c.; if the urine is alkaline, "reverse the order and manner of the change;" and that is all that is to be done to obviate stone in the bladder. It is unnecessary to follow Dr. Gardner further.

**Hancock's Surgery of the Foot.**<sup>1</sup>—This work consists of a series of lectures on the anatomy and surgery of the foot, delivered by

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<sup>1</sup> *On the Operative Surgery of the Foot and Ankle-Joint.* By HENRY HANCOCK, late President of the Royal College of Surgeons of England, &c. 1873.



Mr. Hancock before the Royal College of Surgeons during the years 1865-66, with additions, bringing them down to the present time. Mr. Hancock's name is so well known in connection with conservative surgery as applied to the foot that it is needless for us to say anything to recommend this book to our readers. It contains the results of the author's long and extensive experience on a subject to which he has devoted special attention, and cannot fail to have a powerful influence in encouraging conservatism in the treatment of diseases of the foot.

At first sight the foot seems most unfavorably situated in every way for the performance of conservative operations. The complexity of its anatomy, the frequency with which more than one part of its bony structures is affected, its distance from the centre of circulation, and the apparent hopelessness of the attempt to obtain a solid result after removal of the diseased parts, all tended to deter surgeons from undertaking such operations. The history of the cases here collected, however, clearly shows that good results are attainable, and that a useful foot may be obtained even after the removal of a large portion of its bony structures. As late as the year 1828 it might almost be said that the only operative treatment adopted in this country for disease of the foot was amputation at the "seat of election" immediately below the knee. In that year Chopart's medio-tarsal amputation was introduced. In 1845 Mr. Syme devised his amputation at the ankle-joint, which, although completely removing the diseased member, yet must be considered conservative when compared with former modes of treatment. In 1847 Mr. Wakley removed the lower ends of the bones of the leg with the astragalus and os calcis. Soon after, Mr. Teale removed the os calcis, astragalus, and cuboid bones, the result being a useful foot. From that time, to use Mr. Hancock's words, "surgeons began to shake off the trammels of custom and to think for themselves. They began to consider the beautiful mechanism of the foot, or any portion of it, however small, as something worth preserving." At the present time any manual of surgery will be found to contain directions for at least four distinct amputations and three excisions in the foot, the modifications and variations of which are almost innumerable. In scarcely any other region of the body has so great an advance been made in so short a time, and to Mr. Hancock is due no small share of the credit.

The subject of the various amputations Mr. Hancock treats at great length, and the conclusions at which he has arrived may be summed up very shortly. When it is necessary to remove the whole foot "Syme's operation is unquestionably the best in cases of disease and Pirogoff's for accidents of civil life." Where the astragalus is healthy it may be saved by the performance of Lignerolle's sub-astragaloid amputation, or if both astragalus and os calcis are sound,

as in an accident and no flap is to be obtained from the sole, so as to perform Chopart's operation, a slice may be taken from the under surface of the astragalus, and a portion of the os calcis, similar to that retained in Pirogoff's operation, may be sawn off and applied to its under surface. This operation Mr. Hancock has himself performed with the most satisfactory results. Beyond this point he fully endorses the views of M. Mayor, of Lausanne, who looked upon the foot as a whole, ignoring articulations and sawing through the tissues as near as possible to the disease, removing as little of the foot as circumstances would allow. In Chopart's and Hey's amputations, and in amputation through the metatarsal bones, the plantar flap is the same, only differing in length, and the dorsal flap is so short that a curved incision over the seat of disease will mark it out. It seems, therefore, that in disease of the anterior part of the foot no set operation need be determined on before commencing. We may begin by marking out a short plantar flap reaching to the roots of the toes, which can be lengthened backwards as circumstances may require. The dorsal surface of the bones may be examined by a curved incision, which, if the bones are found healthy at the point, may serve for the dorsal flap. If, on the other hand, they are found diseased the incision may be repeated until healthy parts are reached, when the saw may be applied. With the present bloodless method of operating, this examination can be made almost as perfectly as on an amputated foot, and the slight prolongation of the operation would not be sufficient to counterbalance the advantages of saving as long a foot as possible. In the medio-tarsal operation Mr. Hancock points out the great importance of preserving the scaphoid if possible, or even a small slice of it, for by this means we preserve the attachment of the calcaneo-scaphoid ligament, and so give support to the head of the astragalus, thereby diminishing its tendency to displacement. The only objections which can be raised to the plan of sawing through the tarsus are—1st, the fear of increased suppuration and risk of pyæmia, and 2nd, the liability of the disease to return in the portions of the bones left. Experience, as far as it has gone, has shown that these objections are without grounds.

In excisions of the ankle-joint and the various bones of the tarsus a great deal necessarily depends upon a judicious selection of cases. In the first place it is remarkable how large a proportion of the cases operated on have died of phthisis before the cure was complete; and we cannot doubt that if the operators had been fully aware of the state of the patients' lungs they would have adopted some less exhausting mode of treatment. In the second place it is important to select only those cases in which the disease can be completely removed by the operation. Thus, in excision of the ankle-joint, as Mr. Hancock points out, the disease frequently commences in the



synovial membrane, and only secondarily affects the bones, and in these cases the whole disease is completely removed by excision of the joint. Any argument against attempting the operation founded on the fact that the exact diagnosis is often difficult, and that the bones may be much more extensively diseased than was at first supposed, is without much force, as under these circumstances nothing is more easy than to extend the operation, removing as much as may be necessary, or even to convert it into an amputation. The experience of the operations of Syme and Pirogoff clearly shows that there is little or no danger of extension of the disease in the direction of the bones of the leg. In cases where the ankle-joint is interfered with Mr. Hancock recommends complete removal of the parts entering into the articulation, all partial operations, such as gouging, &c., being extremely unsatisfactory. In planning any operation on the foot it is doubtless well always to bear in mind the chances of failure, and so to plan the incisions as not to interfere with the subsequent performance of one of the amputations at or below the ankle. In excision of the os calcis, for instance, one incision recommended passes across the back of the heel in such a manner that if the operation fails or the disease is found more extensive than at first suspected no resource is left but amputation in the leg.

There are two points to which Mr. Hancock does not allude in this work which seem to us of the greatest importance in the consideration of all operations about the foot—1st, the value of subperiosteal excision of bones, and, 2nd, of the bloodless method of operating. The advantages of this latter are nowhere more marked than in partial operations on the foot. The extent and nature of the disease can be as clearly seen as in a museum specimen, and we should hope the plan of gouging in a deep hole filled with blood, with only the sensations of the finger to distinguish diseased from healthy bone, will be abandoned for ever. From subperiosteal operations we should also expect great things, especially in removal of single bones of the tarsus, such as the cuboid or os calcis. It is possible, also, that saving the periosteum might serve to settle the difference between the supporters of Syme and Pirogoff, as to the superiority of their respective modes of operating. If all the advantages of the slice of bone could be obtained by such means there could be no doubt as to the superiority of such a method. We believe that with careful selection of cases and subperiosteal and bloodless operating, such a degree of success is obtainable in conservative surgery of the foot as not only to justify us in undertaking such operations, but to render it our duty to do so whenever possible.

**Works on Materia Medica.**<sup>1</sup>—These works have been called for primarily by the publication of “additions” to the ‘British Pharmacopœia,’ which they have either incorporated or placed together in an appendix. The same circumstance has likewise led the publishers of the much more comprehensive work, the abridged edition of ‘Pereira’s Elements of Materia Medica and Therapeutics,’ edited by Professors Bentley and Redwood, to reissue copies with these additions printed in the form of an appendix, the several new articles being dealt with in the same way of description and illustration as those in the body of the work.

The characters of these sundry volumes are so well known that a simple notice of their reissue will suffice. The only one of them which might demand further remarks is the treatise on ‘Materia Medica and Therapeutics,’ by Dr. Garrod, inasmuch as it represents an actually new edition, with revised and corrected matter, besides the incorporation of the recent officially recognised remedies in its text. For instance, the new chemical nomenclature has been employed in the formulæ, and “the articles on the physiological and therapeutic action of some of the more important remedies have been enlarged, and in several instances entirely rewritten.”

These improvements are mainly the work of Dr. Baxter, of King’s College, and will be fully appreciated by students, to whom the volume is especially addressed.

**Harvey and his Times.**<sup>2</sup>—It is of the necessity of the case that a Harveian oration should commemorate the virtues and labours of Harvey; and the man would be unworthy the name of an Englishman, and the doctor an ungrateful scion of the profession, who would begrudge praise lavished upon so good and great a man. It is, therefore, right to recall from time to time the excellent work accomplished by Harvey, and to refer to him as an example to those engaged in investigations; but, at the same time, to be called upon to rehearse his merits and good deeds in a public lecture, before a learned assembly already conversant with every detail of his life, imposes no small hardship upon the lecturer to make his address acceptable and interesting. Hence, it has happened that some Harveian orators have, whilst shortly noting the claims of Harvey to perpetual recognition, preferred to accept the discoverer of the circulation of the blood as a type of the philosophic inquirer, and have thereupon proceeded to descant on what a philosopher animated by

<sup>1</sup> *New Editions of ‘Thompson’s Conspectus, adapted to the British Pharmacopœia,’* edited by Dr. BIRKETT; of the ‘*Syllabus of Materia Medica,*’ by Drs. ALEX. HARVEY and A. D. DAVIDSON; of ‘*Garrod’s Essentials of Materia Medica,*’ edited by Dr. BAXTER; and ‘*Pereira’s Elements of Materia Medica and Therapeutics,*’ abridged and edited by Professors R. BENTLEY and T. REDWOOD, with an Appendix.

<sup>2</sup> *Harvey and his Times; the Harveian Oration for 1874.* By CHARLES WEST, M.D., &c. London.



the spirit of Harvey should seek to demonstrate at the present day in the way of new truths, and what direction he should take, in fitting emulation of the philosophic physician commemorated, to arrive at new discoveries.

On his part, however, Dr. West has preferred to follow in the well-worn track, and to sketch the subject of *éloge* and the work he accomplished. This task the able orator has well performed, and his oration will be read with pleasure, even by those already familiar with its subject-matter.

**A Lecture on the Diagnosis of Disease of the Heart.**<sup>1</sup>—This thoughtful and exhaustive lecture on cardiac diagnosis is reprinted from our able contemporary, 'The Edinburgh Medical Journal.' We have nowhere met with a summary of all the circumstances affecting cardiac action and the recognition of heart disease, so concise and yet so complete, as in the pamphlet before us. It will be invaluable to students, and no practitioner can fail, in reading it, to derive instruction and profit.

**An Epitome of Therapeutics.**<sup>2</sup>—The compiler of this book performed for several years the duties of editor of 'The Retrospect of the Medical Sciences,' recently withdrawn from circulation, and, it would seem, has been induced to endeavour to give more permanency to some of the therapeutical memoranda contained in that half-yearly publication, by collecting them in an independent volume.

Without denying to the book some utility in the case of those who have few standard works on medicine to refer to, we cannot assign to it much value. In the first place, the diseases, arranged in alphabetical order, stand for so many certain fixed morbid conditions, possessing a distinct individuality, to be encountered by such and such agents, just as in the books of prescriptions admired by our forefathers, concurrent conditions and complications being more or less lost sight of; consequently the treatment described is directed against names rather than things found in nature.

In the next place the remedial measures noted are heterogeneous, brought together with small critical judgment. This or that practitioner is quoted as recommending this or that drug for the cure of some one condition labelled under a particular nosological term; and the medical man who consults the pages of this book is left to choose for himself from the several therapeutical recommendations before him, with little or no guidance respecting the particular conditions under which the medicament he fixes upon has been administered and found useful. In the instance, too, of many of the modes

<sup>1</sup> *A Lecture on the Diagnosis of Disease of the Heart.* By GEORGE W. BALFOUR, M.D. Edinburgh, 1874.

<sup>2</sup> *An Epitome of Therapeutics, being a Comprehensive Summary of the Treatment of Disease as recommended by the leading British, American, and Continental Physicians.* By W. DOMETT STONE, M.D. London, 1874.

of treatment mentioned we find only the unsupported statements of practitioners, respecting which experience has not attested the correctness. The work, therefore, is rather a contribution in favour of empirical practice than of "rational medicine."

**Stocker's Hints for Health.**<sup>1</sup>—These lectures were delivered before a mixed audience, and consequently pretend to do no more than convey, in a readily comprehensible manner, the general laws of hygiene imposed both upon individuals and on communities. To the public, therefore, in whose interests they were prepared, we can recommend these lectures as full of useful suggestions and of instruction.

**Clarke's Autobiographical Recollections.**<sup>2</sup>—Many of our readers will be familiar with the contents of this volume, which appeared from time to time in the 'Medical Times and Gazette,' and we are persuaded they will be pleased to find them republished as a separate book; whilst those who had not the opportunity of seeing them in our weekly contemporary will be equally gratified to have this chance of their perusal furnished them. To a large proportion of the profession, particularly those members who frequent the medical societies of the metropolis, Mr. Clarke is as much, nay, even more, a "man of the day" than many of the characters pictured in this volume; and they will be gratified to have his autobiographical account of himself, as well as his impressions and notes of the many public and distinguished men with whom he has been brought into contact as a reporter for the medical journals. "Clarke, of the 'Lancet,'" is not a man to be forgotten of this generation. He has catered well, most perseveringly, and usefully, for the profession, and well deserves its recognition and thanks. His merits as the bold coadjutor of Wakley in breaking down the impediments raised by selfishness and avarice, as well as by short-sightedness, in the management of public hospitals and schools, should not be lost sight of, and we are glad he has presented the public with a history of the proceedings which have emancipated medical teaching and hospital instruction from so many of their fetters, and of the part he himself has had in them.

His retrospect extends over forty of the most eventful years the profession of medicine has ever passed through, and in the book before us portrays the principal actors and events from personal knowledge and observation, concerned in bringing about the advancement in medicine in this country, of which, as Englishmen, we may justly be proud.

<sup>1</sup> *Hints for Health; being two Lectures on the Influence of Air, Water, Food and Wine, on the System.* By J. STOCKER, M.D. London, 1874.

<sup>2</sup> *Autobiographical Recollections of the Medical Profession.* By J. F. CLARKE, M.R.C.S. London, 1874.



We heartily commend the volume to our readers as full of interest, and, we will add, also of entertainment. The notice of individuals and of events is written in a graphic though jaunty style, and the serious critic might find occasion for stricture of some expressions and phrases; but both the style and what imperfections it betrays may be set down to the literary habits of the writer as a reporter and purveyor of news to a weekly journal, and to the circumstance he himself apologetically appeals to for the "discursive and gossiping style" adopted, viz. that the book has been "written in the few hours that he could occasionally snatch from a laborious practice."

**The Tongue not essential to Speech.**<sup>2</sup>—This is a thorough and genuine book, likely to be interesting to two, or rather *three*, classes of readers. *First*, the members of the medical profession will find much curious information, proving that (as stated in the title) "the tongue is not essential to speech;" *secondly*, theologians will be glad to see how the supposed miracle at Tipasa (which puzzled two such different minds as Gibbon and Dr. Newman) can be satisfactorily explained; and *thirdly*, the general public may be interested both by the subjects treated of and also by the careful way in which the author collects and arranges his facts, examines the authorities on which they rest, and draws his conclusions.

Into the theological question we need not enter; but as the mutilation of the Christian confessors at Tipasa (A.D. 484) is the earliest instance on record of persons speaking after their tongues had been removed, it will be interesting to try to discover why such an occurrence should have been considered miraculous. In common language, both among the eastern and the western nations of antiquity, the *tongue* was used metaphorically as synonymous with *speech*, as is the case in the present day; and therefore we need not wonder that ordinary persons should be exceedingly surprised when a considerable number of tongueless individuals were heard to speak intelligibly. But the question immediately occurs to us, what would a competent physiologist have said about it? Mr. Fairlie Clarke (in his recent work 'On the Diseases of the Tongue') considers this and several other cases, and sums up the opinions of modern physiologists as follows:

"The accumulated experience of surgeons may now be considered to have given its verdict on the case. We may fully accept the facts, for they come within the domain of natural science, and they correspond exactly with the results which are now being obtained by

<sup>1</sup> *The Tongue not essential to Speech, with Illustrations of the Power of Speech in the African Confessors.* By the Hon. EDWARD TWISLETON. London, 1873. Pp. 232.

surgical operations ; but there is no need to suppose that there was anything miraculous in the occurrence" (p. 198).

It will be interesting to see how far the opinion of an ancient physiologist would have been in accordance with this conclusion. In the first place, it must be borne in mind that cases of excision of the tongue were probably never very frequent, and that the cases on record are exceedingly rare. And therefore we are not aware of any passage in any ancient medical author which shows that the writer knew from personal observation that a tongueless person could speak. But we find in Galen (who represents the highest physiological knowledge of his own and many subsequent centuries) several passages in which the tongue is mentioned in connection with the voice, one of which will be sufficient for us to notice here. He draws a distinction between *φωνή*, *the voice*, and *διάλεκτος*, *articulate speech* ; and says that the organs of the *voice* are the larynx with its muscles and nerves, and that the organs of *articulate speech* are the tongue (chiefly), the nose, the lips, the teeth, the palate, and the uvula ('De Locis Affectis,' iv, 9, tom. viii, pp. 266, 267, 272). With this passage we may compare the words of Sir Benjamin Brodie in a memorandum given to Mr. Twisleton :

"The modification of the voice [*φωνή*] forming articulate speech [*διάλεκτος*] is effected especially by the motions of the soft palate, the tongue, and the lips, and partly by the teeth and cheeks. The mutilation of any one of these organs will affect the speech as far as that organ is concerned, but no farther ; the effect being, therefore, to render the speech more or less imperfect, but not to destroy it altogether" (pp. 108, 109).

The resemblance of the former part of this extract to the passage of Galen referred to above is very close, and upon the whole we are inclined to think that, if Galen had been consulted as to the possibility of a tongueless person speaking intelligibly, he would have answered (though perhaps not without some hesitation arising from the novelty of the case) in the affirmative. Still, we cannot be surprised that the case of the African confessors should have been considered miraculous ; and Mr. Clarke brings forward another instance in which the same explanation was had recourse to, when the Bishop of Caithness (A.D. 1201) exhibited in his own person the same phenomenon (p. 198).

Several cases from the seventeenth century down to the present day are related in detail by Mr. Twisleton, and examined with scrupulous fairness. Indeed, the only fault we have to find with the book is that it contains a very unnecessary amount of details, some of which are of an unpleasantly repulsive character when brought before the eyes of non-medical readers. We should be very glad to see a second edition of a singularly interesting work, in which by compression and omissions it might be made pleasanter to read without at all diminishing its value.



## Original Communications.

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**I.—On Granular Disease of the Conjunctiva and Contagious Ophthalmia.** By EDWARD NETTLESHIP, F.R.C.S., Surgeon to the South London Ophthalmic Hospital. Late Clinical Assistant and Curator of the Museum at Moorfields Ophthalmic Hospital.

DURING the first quarter of the present century the group of diseases now known collectively as contagious ophthalmia received for the first time much attention. It was chiefly in relation to the havoc made by ophthalmia in the English and French armies during and after the Egyptian campaign that the many monographs and articles on the subject which appeared about that time were written. The commencement of destructive outbreaks of ophthalmia in the other European armies at various times between 1814 (Russia) and 1849 (Portugal) drew much attention to the subject on the part of medical men (and especially army surgeons) successively in each country of Europe. As a result partly of the study and skill bestowed on it, and partly of other circumstances, the violent forms of ophthalmia did not continue for long to prevail extensively in any one European army, although this comparative freedom from the disease was not attained till large numbers had been partly or entirely blinded by it in several countries. The importance attached however, both by the medical profession and the lay public, to prevalent destructive ophthalmia, has to a large extent been inherited in the present generation by its milder congeners; by forms of the disease which were common enough long before "army ophthalmia," or "Egyptian ophthalmia," were current phrases. The attention given to these milder and more protracted forms of ophthalmia is, however, due in part to other circumstances than the direct transmission of a newly created interest. Advances made in healthy and morbid anatomy have been the means of attracting notice to changes which could not have received the necessary attention without the light of our fuller knowledge. The superior claims of various very fatal diseases, *e. g.*, smallpox, typhus and dysentery, in earlier days when these were very prevalent in closely packed bodies of people, prevented close attention being given to such relatively unimportant affections as epidemic ophthalmiæ until a

recent time. The higher standard of public health which has slowly grown up, and is still growing, has extended to contagious diseases of the eye as to all other maladies, so that far more importance is attached to the slighter forms of ophthalmia and other non-fatal affections than was the case fifty or sixty years ago.

At the present time ophthalmia, as a disease largely destructive or even injurious to sight, is unknown in the English army, and the number of cases diminishes almost yearly.<sup>1</sup> The same is, in general terms, true of the other European armies. In Great Britain it is chiefly among the children of the poor in large towns, especially of the Irish poor, that ophthalmia is now to be seen; while public attention to it is limited to those moderately severe forms which still occur in some of the poor-law schools.

Notwithstanding the abundant literature of the subject there is still, in this country at least, some uncertainty and want of agreement on the whole question of ophthalmia, more especially as it occurs in large institutions. That ophthalmia has within the last ten or twelve years occurred in a severe enough form to destroy eyes in more than one metropolitan poor-law school; and that in a less severe, but still a serious form, it has been for several years and still is, a very familiar disease in many of these schools, are facts requiring serious attention. It may here be observed, however, that the apparent increase of the disease in some schools within the last few years is doubtless partly due to the fact that much more attention is now given to the slighter forms of ophthalmia in pauper schools than was the case when a lower standard of health was applied to them. For this, among other reasons, the word "ophthalmia," if used without any qualification, is very misleading as to the real amount of harm done by the diseases which it is intended to define, being used, as it often is, to describe cases of every possible degree of severity and of several different kinds. It thus happens in the first place that those to whom the word has a very serious meaning are certain to over-estimate the real amount of the evil if they go no further than the number of cases returned under this heading by different practitioners, while it is probable that, from the want of a uniform standard, cases which one surgeon would regard as ophthalmia might be placed by another in a different category. I shall afterwards show that the number of cases which stand on this debateable ground is, in some instances at least, very considerable.

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<sup>1</sup> Thus, the number of cases admitted into the Royal Victoria Hospital, Netley, for ten years, 1861—70, was 328, 230, 266, 233, 205, 126, 79, 97 (this increase in 1868 was due to the Abyssinian Expedition), 72, 39 (in 1870).—'Army Med. Rep.' for 1870, p. 351. The diminution is attributed to improvements in barrack hygiene at foreign stations. It seems probable that part of it is due also to the fact that since 1868 cases of gonorrhoeal ophthalmia have been returned separately, no such distinction having been made in the returns up to that date. I am indebted to Professor Longmore for this information.



Then there is still much uncertainty as to the percentage of cases in which permanent damage to sight and incapacity for earning a living result from that kind of ophthalmia known as "granular." That bad granular ophthalmia often gives rise to very serious permanent corneal damage is of course undoubted. It is not, however, equally clear that the disease, even when severe, gives such a large percentage of bad results as is often attributed to it; and it is very doubtful whether, when mild or moderate, it is so serious an affair as we are sometimes led to believe. On the other hand, it is probable enough that some to whom opportunities for studying the disease on a large scale have been wanting are not fully aware of the injury caused by it. The principles of therapeutic treatment are well founded; there are yet a few points to be determined, however, with respect to the details which are most suitable, the length of time that treatment should be kept up, and the effects it should be expected to produce. It is also desirable to settle in as much detail as possible what are the best hygienic and educational measures to be taken in respect to large collections of children suffering from the disease.

The contagiousness of many of the forms of conjunctivitis has long been settled beyond dispute. It is indeed a question (and one of some importance for administrative purposes in large establishments) whether the doctrine of contagion has not been pushed too far with some forms of conjunctival disease, and made to account for facts which are in reality due to other causes. Whilst it is essential that the prevalent opinion on this subject should err, if at all, on the safe side, it is very desirable that this error should be as small as possible, and that a safe and sufficient prominence should be given to other influences. Some light is thrown on the causation of the diseases in question by an examination of their early history in this and other countries, especially as to the alleged freedom of Europe from severe prevalent ophthalmia up to the date of the Egyptian campaign of 1798 to 1802. The possibility of entirely preventing or stamping out ophthalmia from large institutions also depends much on the causes which produce it, and our belief on the latter subject will of necessity influence our action with reference to the former. The fact that ophthalmic hospitals do not furnish opportunities for a prolonged study of the various cases of conjunctivitis has prevented a full investigation of these diseases by some of those who have devoted themselves especially to ophthalmology; while the very large and prolonged experience of the disease enjoyed by many army surgeons and by the medical officers to some of the poor-law schools furnishes most valuable material for completing the unavoidable deficiencies in the experience of oculists. It is by putting the opinions of these two classes of practitioners side by side that we may hope to gain a sufficiently full and exact knowledge of the

disease without over-valuing its importance on the one hand, or bringing to bear on it insufficient means of therapeutic treatment or general management on the other.

The action taken in 1873-4 in reference to ophthalmia in the North Surrey District School at Anerley gave me, as Medical Superintendent of the Branch School established for the temporary reception and separate treatment of the patients, the means of studying those forms of the disease which are now most prevalent in such schools. I had under care for twelve months nearly 400 cases of the disease in its various milder phases, and had also the opportunity of observing to some extent the conditions under which it occurred at the Anerley School. These facilities together with a few opportunities which have occurred of inspecting the eyes of children in other schools, and the knowledge gained by attending the practice at Moorfields, have placed me in a perhaps somewhat unusually good position for judging some of the points above mentioned in connection with those forms of ophthalmia which are most prevalent in this country at the present time.

Dividing diseases of the conjunctiva roughly into those which are at some part of their course acute and those which are chronic throughout, we have in the former group the various forms and degrees of conjunctivitis, while granular ophthalmia (trachoma) is the most important member of the latter.

The fact, which has now been clearly known for about thirty years, that a person affected by granular disease of the conjunctiva is likely to suffer from a superadded conjunctivitis much more severely and for a much longer time than if his conjunctiva had been healthy when the conjunctivitis set in, gives the disease much greater importance than it would otherwise possess. This is recognised more or less clearly in all modern text-books, but is still sometimes lost sight of in the complications due to the frequent occurrence of the two types of disease simultaneously in the same patient. There are many cases of the milder forms of acute conjunctivitis which are of very little importance when they occur in persons whose conjunctiva was previously healthy, but which have serious results if at the time of their attack this membrane was in a more or less granular state.<sup>1</sup> A third disease which, though not properly conjunctival, sometimes exerts an important influence for harm on conjunctival affections is the chronic inflammation of the follicles of the eyelashes and of the meibomian glands and neighbouring struc-

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<sup>1</sup> Although the word "granular" is here, and throughout this paper, used to designate the disease whose essential feature is the "vesicular" or "sago-grain granulation," I would not be understood to doubt that prolonged or repeated inflammation of a previously healthy conjunctiva, either with or without implication of the eyelash follicles and Meibomian glands, may give rise to permanent thickening and some roughness of the palpebral conjunctiva. I should doubt, however, whether this is common in any but old people.



tures which furnishes the different types of ophthalmia tarsi. It is, so far as I have seen, less common in schools than the two first classes and there is no need to discuss it at length here since its characters and management are well known.

*Granular disease of the Conjunctiva.*—For practical purposes it is convenient to treat this part of the subject first, especially because in the patients from whom most of my experience has been drawn it is so common that from a clinical point of view it almost must be studied first in order to appreciate rightly the bearings of the two classes of disease on one another.

Granular ophthalmia is a specific disease characterised by the enlargement of the minute lymphatic follicles normally present in some parts of the palpebral conjunctiva, by the formation of new structures which, so far as clinical characters are concerned, are identical with the above, and later by the slow hypertrophy of all the other conjunctival structures. The earliest changes occur at the outer<sup>1</sup> part of the lower lid near to the oculo-palpebral fold; at a later period the whole lid is affected and similar changes are found on the tarsal part of the upper lid and on the part of the conjunctiva which is above the upper edge of the tarsus. In the upper lid the disease begins at the extreme outer or inner end, or at both simultaneously, gradually advancing from these two points until the greater part or the whole of the tarsus is affected. In very mild cases the upper lid is scarcely involved.

The enlarged follicles are described in their early stage as “vesicular” granulations. They are at first quite transparent and only just visible to the unaided eye; their contents are said to be fluid enough to flow out when they are pricked. A little later on, when they are somewhat larger, less perfectly transparent, and will no longer collapse, they are called “sago-grain” or “spawn-like.” They are sometimes compared to tapioca when, at a much more advanced stage, there are present at and above the upper border of the superior tarsus large tracts or mounds of greyish white, almost opaque, new tissue formed either by the coalescence of separate granulations or the diffuse infiltration of the conjunctival tissue with new material. From the fact that much adenoid tissue is present in the tarsal conjunctiva and oculo-palpebral fold in certain very young animals<sup>2</sup> before any lymph-follicles are developed, and that

<sup>1</sup> Dr. Littlejohn, resident medical officer to the Hanwell poor-law school, whose opportunities for observing this disease in its earliest stages are very large, tells me he thinks the first signs of disease are found in the minute vesicular-looking elevations so often seen forming a single line parallel to and about  $\frac{1}{8}$ -inch from the border of the lid at its outer part. I have been in the habit of looking on these as of doubtful diagnostic value. They will repay further study.

<sup>2</sup> Diffuse adenoid tissue is stated by Schmid to form the chief part of the conjunctiva palpebrarum and oculo-palpebral fold in many very young animals. Blumberg says nearly the same with respect to the dog. (*Stricker's Manual of Histology*, New Syd. Soc.'s Translation, vol. iii, p. 450).

these are formed from it somewhat later in life (third week), it seems possible that in after life a formation of similar diffused tissue may occur as the result of disease.<sup>1</sup>

The disease may advance no further than the formation of sago-grains on the conjunctiva of the lower lid, with or without a slighter degree of the same change in the upper lids. In such cases there is often no increased congestion of any part of the palpebral or ocular conjunctiva, nor any subjective symptoms. There is in many cases, perhaps in most if the truth could be ascertained, a slightly increased secretion of mucus, just enough to remain as a film on the skin at the inner or outer canthus when the tears which often run over at waking have dried up. The "sago-grains" may thickly cover the lower lids and yet there may be no morbid redness whatever. Such a state, few or many indolent follicular granulations without any perceptible change in the other conjunctival structures, will often remain stationary for a long time (many months or a year certainly, probably several years) and lead to no further damage. After a time the granulations gradually change to a yellowish or brownish-yellow colour, slowly shrink and at last disappear, their site being often occupied for some time afterwards by a small rusty-brown dot or pucker in the mucous membrane from which two or three minute but somewhat enlarged blood-vessels may be seen to pass. In other cases minute ill-defined whitish dots or streaks (scars) mark the former position of the morbid growths.

The formation of sago-grain granulations is often accompanied, either at an early stage or after they have attained a moderate size and number, by enlargement of the papillæ and chronic congestion and thickening of all the other conjunctival structures, including probably the tubular mucous glands which in man are found in the oculo-palpebral fold and some parts of the tarsus. The follicular granulations at the same time become congested and larger, though they may be partly or entirely hidden in the swollen tissues and hypertrophied papillæ around them. The conjunctival tissue in many cases takes part in the sago-like change, becoming much thickened by greyish gelatinous-looking material. When this occurs the sago-grains attain a large size and may coalesce with one another, and we then find an enormously thickened conjunctiva covered with bossy greyish-red elevations (large sago-grain granulations) separated by florid or dull-red enlarged papillæ. The entire membrane, being much extended in area, often becomes folded on itself to a remarkable extent. The degree of thickening and the size of the follicular granulations is sometimes very unequal on different parts of the same lid. I believe that the final stage of such a condition, whether affecting the whole lid or only a part, is always

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<sup>1</sup> A similar suggestion has been made by Frank (1860) and others.



the partial absorption of the new tissue and the conversion of the rest into tough scar-tissue of white or yellowish-white colour. This will form a more or less isolated streak or plate, or will occupy the greater part of the lower lid, according to the original extent of the trachomatous change. Such plates are sometimes slightly raised and at others a very little depressed; they are always less flexible than the normal conjunctiva. When thin they may have a streaky, tendinous appearance. Sometimes by slow contraction they cause stricture of the lachrymal puncta or canaliculi,<sup>1</sup> or both, and more or less troublesome epiphora is the result. Often they do no harm whatever. I have never seen them, in children up to sixteen years old, cause alteration in the direction or curvature of the edge of the tarsus, except very slightly at the punctum.

Granular disease, as a rule, begins rather later on the upper than on the lower lids. In cases of the first degree, where there is throughout little or no congestion and papillary enlargement on the lower lids, the upper lids generally remain nearly healthy from beginning to end. In these mildest cases, however, a few small sago-grains are often found close to the inner and outer ends of the upper tarsus. They are generally accompanied by more or less congestion and papillary enlargement at the same spot. Sometimes the papillæ enlarge without any sago-grains being visible, either because these are not formed, or being present are obscured by the papillæ. The rest of the upper tarsus and retro-tarsal fold is often normal. In the next degree either the whole length of the upper edge of the tarsus shows abnormal redness with some papillary enlargement, or the entire tarsal surface loses its polish, and, owing to uniform slight enlargement of its papillæ, reflects light imperfectly. Both these changes occur together in many, perhaps in most cases. Now and then, although there may be only the very slightest papillary change, the tarsus is found to be thinly sprinkled with small circular flattish elevations like minute sago-grain granulations. They are quite pale like the surrounding conjunctiva and can scarcely be detected except by looking obliquely at the lid. It is commoner, however, to find no appearance of sago-grains (except just at the inner and outer ends) until there is slight but quite decided papillary enlargement and congestion over the whole upper tarsus. The surface is then of a more or less uniform red tint, sprinkled over with small white bodies of a flattened hemispherical shape. The smallest are only just visible to the naked eye, the largest nearly a millimètre in width. They are absent from that part of the lid nearest to the ciliary border, and are less plentiful near the upper border of the tarsus than about its central area. Sometimes they extend quite up to and mingle insensibly with the sago-grains before men-

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<sup>1</sup> This occurred in three cases.

tioned at the inner and outer ends of the lid; in other cases they are limited, or almost so, to the central part of the tarsus. Occasionally only one or two are present on the whole upper lid. They are generally smaller, and almost always flatter, than the granulations at the canthal parts of the lid or than those on the lower lids. These small sago-grains on the upper lid cannot be mistaken for the enlarged and congested papillæ by which they are surrounded and which give to the conjunctiva the appearance of the finest sandpaper. They are, however, not by any means always present on the general surface of the upper tarsus, for in a considerable proportion of cases there is nothing to be seen but uniform congestion and the finest sandy roughness of this part. This congestion may be uniformly capillary, the larger vessels being quite hidden, or the larger vessels may be the more obvious, there being then coarse congestion without much alteration of the normal smoothness of the conjunctiva. It is worth noticing that the congestion affects last and when not uniform is slightest at, a small linear part of the tarsus situated midway between the two ends and about 2 mm. from its ciliary border. The large vessels all converge to or radiate from this area, which is exactly the part where, as we shall see later, scarring begins in severe cases of granular lid. In the later stage of a slightly or moderately granular upper lid there is a varying amount of subconjunctival thickening, considerable papillary enlargement, usually much congestion and many whitish more or less prominent sago-grains. In the place of some of the latter small slightly depressed pale streaks or dots are often to be seen, indicating absorption of previously existing granulations. The whole lid at this stage has a peculiar mottled colour and irregularity of surface which are highly characteristic.

The relative development of the papillary and sago-grain changes vary greatly in cases of the degrees above described. In a good many instances all the papillæ are so much elongated as to form a surface like the pile of velvet, different patches or rows of papillæ often separating from one another and leaving fissures which appear at first sight to be cracks in the mucous membrane. There may be no sago-grains visible on the upper lids even if any are present, and those on the lower lids may be almost or quite hidden by the velvety hypertrophy of the papillæ. That the essential element in the disease, even in these cases, is the sago-grain granulation is I think certain in almost all cases. In most *early* cases sago-grains are obvious enough on the lower lids at least, and in many of these velvety cases they reappear much more clearly during conditions of partial cure when the papillæ have much diminished in size. The natural ending of the velvety type of cases is in a gradual condensation of the over-grown papillæ, followed by shrinking and devascularisation. There is at last left a pale surface, perhaps partly scarred



and quite smooth in some parts and in others occupied by small, pale, tough, firm papillary granulations, looking like coarse sand. These are most abundant and largest at and above the retro-tarsal fold of the upper lid. They often remain in this state for years, probably for life.

Hitherto the three elements in the disease, follicular granulations, hypertrophied papillæ and diffuse (? adenoid) infiltration of the conjunctiva and subconjunctival tissues, have all been more or less completely appreciable by obvious differences. Although the relative amount of each change has been seen to vary largely, still the sago-grain (lymphatic follicle) granulation is the primary and essential constituent in every one.

In the severer forms of granular ophthalmia, although the sago-grain granulation is an equally important primary change, the morbid action extends so largely to the papillæ and the other conjunctival structures that it is often quite impossible to distinguish between follicular and papillary granulations. The papillæ, which in an early stage of disease are increased more in length than width, become in the more aggravated conditions gradually thickened, until they form rounded masses indistinguishable either by size, colour, or apparent consistency from the true sago-grain bodies. In some cases the papillæ may be seen in all stages of enlargement on different parts of the same lid at the same moment. The subconjunctival tissues now always become more or less infiltrated with inflammatory material. The appearance of the conjunctiva at this stage, its surface thickly studded with large florid succulent rounded "granulations," needs no description. It is in this state that most of the patients seek hospital relief.

The final absorption of the effused products in these bad cases is always accompanied by the condensation of a part of it into firm, white, tendinous cicatricial tissue. It is unnecessary to describe in detail the form and arrangement of these scars, further than to mention that the chief one is almost always found as a linear patch or band midway between the inner and outer ends of the upper lid, between the centre and the free border of the tarsus and parallel with the latter. The precedence taken by this part in the retrograde changes is no doubt connected with the fact that it is the least vascular part of the lid. This point, as has been already mentioned, is beautifully shown in many upper lids when in a state of moderate chronic congestion. The depth of scarring depends chiefly on the degree to which the subconjunctival tissue has been implicated in the inflammatory process. Bad results from changes in the curvature of the lids and permanent shortening of the conjunctiva are likely to follow in proportion to the *depth* of the scar, and in proportion as it affects the oculo-palpebral fold. It must be distinctly borne in mind that scarring is the natural termi-

nation of granular disease, when at all severe, if not interfered with. From the constancy with which it occurs in cases of the same degree of severity, from the invariable occurrence of the chief cicatrix in exactly the same part of the lid and this the least vascular part, and from the fact that treatment of precisely the same kind as has been used in severe cases never produces similar scarring in mild ones, there can, I think, be no doubt whatever that most of the cicatrices commonly met with now, especially the worst ones, are due to the disease having been allowed to go on without sufficient treatment, not to the use of too powerful caustic applications.<sup>1</sup> The scars following the spontaneous cure of granular ophthalmia may be fitly compared to those following the disappearance of non-ulcerating deposits of lupus, or the spontaneous cure of nævi.

*Symptoms accompanying Granular Ophthalmia.*—There is the greatest variation in the subjective symptoms of this disease, a variation which bears no relation at all, in *different cases*, to the objective changes, although in the same patient this relation remains almost always constant. The great majority of children with abundant sago-grains on their lower lids and often a good deal of congestion and papillary hypertrophy, will tell you they have nothing the matter with their eyes. Generally there is no photophobia whatever. There is often more or less drooping of the upper lid from elongation of the tarsal ligament and this gives the patient a sleepy aspect. This total absence of discomfort is quite common even in much more advanced cases where the conjunctiva of the upper and lower lids is crimson, thickened and converted into a mass of mingled follicular granulations and enlarged papillæ. There is no redness of the exposed part of the ocular conjunctiva, but often, perhaps generally, the parts ordinarily covered by the lids are more or less congested. This condition is most intense at the oculopalpebral fold where it merges into the redness of the palpebral conjunctiva; gradually diminishing, its last traces are seen in a few of the larger conjunctival vessels as they straggle towards the cornea. The state of congestion of the palpebral conjunctiva is

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<sup>1</sup> Scars do sometimes result from ulceration of the conjunctiva produced by treatment, but these are different both in position, appearance, and character from those due to the natural cure of the disease. They are generally found on the lower lid near its centre in the form of short wide bands joining together two principal folds of the conjunctiva which is in this position very lax and often much plicated. They are always preceded by superficial ulceration, and this generally takes place on the two opposed surfaces of a sulcus in the folded conjunctiva, and is the result of a too free application of nitrate of silver or lapis divinus to that part. I have never seen any distortion of the edge of the lid follow them. It very occasionally happens that similar ulceration and scarring occur on a small patch of the upper lid from the same cause; these scars are, however, abruptly circumscribed, quite superficial, devoid of the peculiar tendinous glistening of the spontaneous scars, and especially have no uniform position on the lid.



often very accurately shown by that of the caruncle, which by its red fleshy aspect often reveals a condition that might otherwise escape notice. These remarks on congestion apply to children seen in the daytime. In examining a number of cases just after waking in the morning it is not unusual to find several with some congestion of the exposed ocular conjunctiva. This soon disappears, a result probably due in part to the constringing effects of exposure to cold air.<sup>1</sup> The permanent congestion of those conjunctival vessels which are constantly covered by the lids is caused partly no doubt by their being kept constantly warm, and partly also by their greater proximity to the chief blood-supply than those on the exposed part of the globe. In some cases there is photophobia; this varies in amount at different times in each patient, but generally remains for a very long time attached to the same cases. As far as my observation has gone photophobia generally bears some relation to the former or present existence of corneal ulceration. Probably in some cases constant winking due to photophobia (or more correctly to exalted sensibility of the conjunctiva) causes the ulceration in the first instance. In others an attack of phlyctenular ophthalmia sets up the intolerance of light for the first time. It is maintained in some cases partly by habit; whilst, on the other hand, the act of winking helps to keep up the conjunctival irritability which first gave rise to it.

The chief symptom remaining to be noticed is that of *discharge*. It is not quite so easy as it appears at first sight to prove the complete absence of discharge from the conjunctiva in a large collection of children. If there is any at all, it will be found early in the morning, before the face has been washed. It may either glue the lashes together into little pointed bundles, or if mixed with much tears may flow over the skin of the lower lid and dry into little

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<sup>1</sup> Nocturnal attacks and exacerbations. Several interesting facts may be mentioned here. All observers agree in stating that attacks of purulent ophthalmia generally come on during the night, *i. e.* that the earliest symptoms are noticed on waking in the morning. It is also commonly mentioned that cases of acute ophthalmia of all kinds, including relapses, are worse in the morning than at any other time. This extends to the slightest cases and I have often noticed children who showed slight ocular congestion in the morning get quite well during the daytime. Even in health the eyes of many persons are a little "blood-shot" in the morning. The fact has often been used as evidence that contagion generally occurs during the night and through the medium of the air. It does not seem to me of much value for this argument, since the inoculation on this hypothesis must occur during the half hour or hour before the patients go to sleep. It appears much more likely to depend (putting aside cases due to cold wind) on the increased supply of blood which one would suppose must flow to the eyelids when the body is horizontal and the conjunctival vessels relaxed by the warmth following the closure of the lids in sleep. These would be the best conditions for the multiplication of a germ implanted during the previous day. No doubt the morning exacerbation during acute attacks is partly owing also to the irritation caused by pent-up discharge and tears.

white flakes like the dried slime of a fish, or may collect into a little pellet at the inner canthus, or may lie concealed in the form of a string or flake far back between the lower lid and the eyeball. From many careful and repeated observations of slight cases I have no doubt that in a large proportion of those whose palpebral conjunctiva is congested there is a slightly increased secretion of mucus. For practical purposes it does not appear of much consequence whether this mucous discharge proceeds from the hypertrophied tubular glands of the conjunctiva (Müller, Thiry), or from the epithelial cells shed from the general surface; probably it owns both these origins. It is of more importance to notice that it may from various slight causes become opaque and white or yellow from admixture with pus, that this change in quality (which is always accompanied by a slight increase in quantity) often takes place very quickly (in a few hours), and that it may quickly resume its former character and quantity. There is also sometimes increased secretion of tears so that some children's eyes always look "watery." The two kinds of secretion (mucus and tears) are not present in corresponding quantities; in some the discharge is always "watery," in others always "gummy." A great many cases never go beyond the following symptoms:—slight mucous and watery discharge; occasional slight ocular congestion and perhaps a little transient photophobia; constantly more or less congestion of the caruncle and of the larger remote<sup>1</sup> ocular conjunctival vessels. These may coexist with very granular lids. The course of the symptoms and the limits within which they vary, are as a rule remarkably constant in each child; but there is no fixed relation between them as occurring in different children. It is certain that many children with badly granular lids continue for long (probably several years) to show a little discharge, and that this in the same child will repeatedly go through the same changes, becoming at one time slightly purulent for a few days and then again diminishing to a mere trace of translucent mucus, and remaining in that state for weeks. There are others who (if not under treatment) constantly have rather more discharge, so that their lids will be generally glued together in the morning and more or less discharge will collect within the lower lids or at the inner canthi during the day. Beyond these, again, is another group in which the discharge, as a rule, gradually increases soon after treatment is omitted and at length becomes profuse and purulent, accompanied by ocular redness and often by photophobia; here, as in the slightest cases, the symptoms bear no fixed relation to the *degree* of granular disease. Whether the discharge in these two groups of cases is to be looked upon as the normal result of the uninterrupted granular disease, or, on the other hand, as having

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<sup>1</sup> *i. e.* remote from the cornea.



originated in a superadded contagion or in some irritation of the conjunctiva, the cases are for clinical purposes distinct from those in which there is never any, except very slight mucous, discharge, and will be best considered under the subject of contagious ophthalmia. It may be mentioned here, however, that the majority of these cases of persistent discharge occur in young children (from three to eight), and that from the constancy of the symptoms in each case it is almost certain that the gradual relapses which occur in them, after treatment has been omitted, are due to the return of that morbid action on the part of the conjunctiva which had been much reduced and kept in check by local applications.

*Termination and results of the Granular Disease.*—There is much difficulty in ascertaining the proportion of cases in which permanent injury to sight is a result of granular lids, and notwithstanding all that has been written there is, I think, still some misapprehension on the subject. While fully admitting the very serious consequences to sight which result from ulceration of the cornea and pannus in bad cases of granular lids and going so far as to say that a considerable proportion of the opacities met with in children are due directly or indirectly to this disease, I think it is an error to assume that such consequences follow the majority of cases of granular lids. From what has come under my own observation I can have no doubt that the opinions expressed on this subject in various forms by many army surgeons are substantially correct and that most cases of moderate or even tolerably severe granular ophthalmia pass through their stages, recover, with or without scarring, and are not afterwards followed by any harm whatever. No doubt almost all cases of partial and complete pannus and of organic entropion, and some cases of trichiasis and of changes in the position and size of the lachrymal puncta and canaliculi, are the results of granular ophthalmia. So long as the lids, especially the upper lids, are considerably roughened, either by chronically enlarged papillæ alone, or by a mixture of sago-grain and papillary granulations, or by prominent rigid cicatrices, there is always a risk that they may set up ulceration of the corresponding part of the cornea with development of vessels in its superficial layers. Cases of repeated superficial ulceration of the cornea, resulting in numerous facets, are also not uncommonly the result of granular ophthalmia complicated with severe ophthalmia tarsi. There is also the risk that, short of these results, granular disease may bring on prolonged and repeated photophobia of a degree quite enough to interfere with everyday occupations. When all these have been allowed for, however, there will still remain a large proportion in whom no serious results happen during the several years that the children stay in the schools. It is stated that entropion comes on as long as ten or twelve years after chronic granular ophthalmia (Snellen). Without questioning that this may occur in

certain cases, I venture to think that without much qualification such a statement is apt seriously to mislead. I have seen a great many children whose eyelids are extensively scarred by this disease, some of them having certainly (from their history) been in this condition for several years, who suffer no inconvenience and in whom, though I have had them under constant observation for twelve months, not the slightest alteration has occurred in the curvature or position of the lids.

From the very long course of the disease and the erratic habits of patients, it is practically out of the question to follow cases to their termination in civil hospital practice. The same objection applies in a great measure to poor-law schools, where the children are sent out to service when fit and of proper age and it becomes difficult to trace them for more than a short time afterwards. Some of the worst cases come under care again at the ophthalmic hospitals; but then it is not of the worst cases that there is any question. It is in respect to the cases next in order of severity, where there is disease enough to produce permanent change in the structure of the palpebral conjunctiva and subconjunctival tissue and not infrequently to invalid the patient for a long time unless under treatment, but in which the permanent detriment to the cornea is generally either slight or nothing, that the completion of the clinical history is most wanted. Cases of this degree form a very large majority of those who come under the surgeon's hands for ophthalmia in our pauper schools, and probably also in the army. It is likely that the older inmates of workhouses would furnish as fair a field for inquiry on this question as could be found. Among them we should not expect to find many persons to whom the disease had proved extremely serious, for probably in many of these life would be indirectly shortened by the malady while others would obtain provision from various charitable bodies or by pensions. If it were the case, however, that any considerable number of poor persons had once suffered from the disease in tolerable severity but had not received from it any permanently disabling result, we might expect to find a fair sprinkling of them among the ordinary old and "infirm" inmates. A full inquiry of this kind would occupy much more time than has been at my disposal for the purpose, and I therefore give the results of such inquiries at two of the workhouses rather as indicating the direction in which a useful investigation might be made than for the purpose of drawing final conclusions. So far as they go, however, the cases given below confirm the hypothesis that attacks of granular ophthalmia (complicated or not with purulent ophthalmia), severe enough to give rise to considerable scarring of the tissues of the upper lid, are often not followed by any remote ill consequences from the slow contraction of the scar.

The following table shows the remote effect on the curvature of



the eyelids, direction and growth of the eyelashes, and functions of the lachrymal puncta and canaliculi, in twenty-one persons (for the most part forty years old and upwards) whose eyelids showed more or less scarring the result of previous attacks of granular ophthalmia. The inquiries were made at two metropolitan workhouses, from the medical officers of which I received permission to examine the eyelids of every inmate. The method adopted was to ask each person whether he or she had ever had "bad eyes" of any kind. If the answer was "no," and there was no distortion of the eyelashes, epiphora, thickening and redness of the edges of the lids, or discharge, I usually did not evert the upper lids. Whenever, from the answers given or from external signs, a previous attack seemed probable, I always examined the inner surface of the upper lids. In almost every inmate I looked at the inner surface of the lower lids.

Besides the cases tabulated below, which include all in which there was scarring of the palpebral conjunctiva, there were a good many cases of the chronic ophthalmia common in old people, characterised by thickening of the free borders of the lids with or without alterations of the puncta and lashes, sometimes with muco-purulent discharge and chronic thickening of the palpebral conjunctiva, but in no case with scars on the inner surface of the lids. These are not included in the table.

In examining the upper lids for conjunctival scars it is necessary to be aware that the appearance of an incipient linear cicatrix in the typical position already mentioned may be rather closely imitated by the blanching, due to pressure, which often occurs at exactly the same part while the lid is everted. Now and then I have found it almost impossible to be certain whether there was or was not any actual scar, but in the great majority of cases it is only necessary to be aware of the fallacy in order to avoid it.

The number of inmates so examined was about 400 in one workhouse and 450 in the other. The great majority of those examined were more than fifty years old.

| No.<br>Sex.<br>Age. | Race.                             | Scarring of upper lids; extent, &c.   | Date of attack.  | Present effect on margins of lids<br>and lashes, &c. | State of corneæ.  |
|---------------------|-----------------------------------|---|--|--|---|
| 1—<br>F.<br>68      | Irish.                            | Both upper lids and both lower lids.  | "Blight" 15 years ago; never before.   | None.  | Old nebulæ.   |
| 2—<br>F.<br>87      | Irish.                            | Both upper lids very extensively scarred.   | Bad eyes on and off since age of 20.   | Slight sycosis tarsi; no distortion.                 | Clear (has cataracts).                                    |
| 3—<br>F.<br>39      | Irish.                            | Right upper lid scarred, and still somewhat rough.  | Eyes bad for first time 19 years ago (at age of 20); never before.   | None.  | Right somewhat vascular and nebulous; left very nebulous. |
| 4—<br>F.<br>70      | Irish.                            | Both upper lids slightly scarred, and still some roughness.   | Cannot tell; memory bad.   | None.  | Clear.  |
| 5—<br>F.<br>68      | English.                          | Right upper lid scarred.  | Says eyes were never bad till 2 years ago; probably incorrect.   | None.  | Clear.  |
| 6—<br>M.<br>50      | Born in London.<br>Parents Irish. | Both upper lids well-marked scars, still some roughness where not scarred; minute scars on lower lids also. | At æt. 10 had bad eyes for 2 years in a workhouse-school; got well and earned a good living for self and family. At æt. 35 had second attack and was disabled for 2 months; since then well. | None.  | Clear.  |
| 7—<br>M.<br>86      | English.                          | Slight scarring of right upper lid; both upper lids red and thickened by papillary granulations.            | "20 years" ago had "sore eyes" on board a man-of-war (probably more than "20 years" ago).  | None.  | Clear.  |



|                |                                  |   |  |  |   |
|----------------|----------------------------------|---|--|--|---|
| 8—<br>F.<br>48 | English,<br>but father<br>Irish. | Well-marked scar of left upper lid; right eye lost and shrunken, probably by a violent attack of same disease, but attributed by her to "nitrate of silver," running into it while the inflamed left eye was being treated. | Bad eyes 31 years ago (at æt. 17); were bad on and off till æt. 26; since then no trouble at all.  | None.  | Left clear.   |
| 9—<br>M.<br>68 | English.                         | Right upper lid slightly but definitely scarred.  | 51 years ago (æt. 17) inflammation of right eye for "6 months" on board ship.  | None.  | Right cornea slight central haze and a very marked arcus at upper part; left clear, except moderate arcus. Large diffused haze of each, but no vascularity. |
| 10<br>M.<br>33 | Irish.                           | Slightly irregular but definite scarring of both upper lids.  | Eyes bad about 13 years ago for 3 years, while in New Zealand Army; discharged from Army for defect of sight in consequence. Operation (probably peritomy), by Mr. Hutchinson, 8 years ago; eyes as good as now 7 years. | None.  |   |
| 11<br>M.<br>68 | Irish.                           | Well-marked scar of both upper lids; rest of each lid somewhat rough.   | Does not know that he ever had bad eyes.   | Slight marginal blepharitis; edge of each upper lid a little straightened by shortening, but no inconvenience. | Clear.  |
| 12<br>M.<br>60 | Irish.                           | Well-marked scars on both upper lids.   | 30 years ago (at æt. 30) had bad eyes for "10 weeks."  | None.  | Slight haze of a narrow segment of upper part of each, but no interference with vision.   |
| 13<br>M.<br>70 | Irish.                           | Very extensive scarring of both upper lids.   | 17 years ago had "bad eyes," brought on by getting a "blade of wheat into left eye whilst in the harvest-field;" never before or since.  | None.  | Left, large leucoma and anterior synechia from perforating ulcer; right, slight central haze and probably old iritis.                                       |

TABLE—continued.

| No.<br>Sex.<br>Age. | Race.                         | Scarring of upper lid; extent, &c.   | Date of attack.   | Present effect on margins of lids<br>and lashes, &c.  | State of corneæ.                             |
|---------------------|-------------------------------|--|---|---|--|
| 14<br>M.<br>73      | Irish.                        | Patchy scarring and old granular lids; all the lids intensely congested.   | Bad attack 3 years ago, "could not see for 3 months;" was under treatment; eyes liable to "stick" in the morning for about 12 years.  | None.   | Clear.                                       |
| 15<br>M.<br>64      | English.<br>Parents<br>Irish. | Very extensive scarring of both upper and both lower lids.   | "Blight" 10 years ago, whilst in a workhouse; "15 others" had bad eyes at same time; his eyes were bad for "5 years, and for the last 5 years or so have been as well as now, and free from discharge;" says emphatically that he had no treatment of any importance. | None (perhaps slight shortening of the oculo-palpebral part of conjunctiva, but not enough to do any harm). | No note.                                     |
| 16<br>M.<br>30      | English.<br>Parents<br>Irish. | Left upper lid only has well-marked linear scar in usual place; lower lid nearly normal; right lids normal.      | 8 years ago inflammation of left eye after getting line into it.  | None.   | Large old haze from inner margin to centre.* |
| 17<br>M.<br>37      | English.                      | Slight scarring in usual position in both upper lids; much redness and thickening of whole conjunctiva.          | 7 years ago had inflammation; fresh attack lately.  | Slight eversion of edge of each <i>lower</i> lid; bad old syco-sis tarsi and loss of lashes.                | Old central haze of each.                    |
| 18<br>M.<br>22      | English.†                     | Superficial reticulated scars of all the lids and in left upper lid a well-marked linear scar in usual position. | Was never in the infirmary for "bad eyes" and does not admit that they ever were bad.   | None.   | Perfect.                                     |



|                |          |   |   |   |  |
|----------------|----------|---|---|---|--|
| 19<br>F.<br>41 | Irish.   | Slight scar on right upper lid in usual position; conjunctiva of all the four lids very red and thick but not granular.                         | No definite history of inflammation; probably began as sycosis tarsi, which has spread to the conjunctivæ; left eye "hazy" for "12 years."                        | Entropion of both upper lids; left the worse.†                            | Left, a diffused haze.   |
| 20<br>F.<br>53 | English. | Right upper lid extensively scarred all over the tarsus, and scar especially dense in usual position; lower lid also much superficial scarring. | 7 years ago both eyes bad for "4 months," right was worse; treated by "caustic" to insides of lids, at Charing Cross Hospital; since then quite well; no relapse. | None.   | Extensive arcus, wider at the upper segment, and probably there in part due to former ulceration from granular lids. |
| 21<br>F.<br>43 | Irish.   | Moderate scarring of right upper lid in usual position; still some old pale papillary granulations on both upper lids.                          | "20 years" ago eyes "weak and watery" for "12 mos.;" never very bad.  | Edges of lids thick, and lashes a little distorted, but no inconvenience. | Clear.   |

■ The corneal haze here is doubtless due to direct injury by the lime. Probably the scar on the lid being in exactly the position for granular ophthalmia is due to that disease, greatly aggravated and made active by the lime injury.

† Brought up in a London workhouse-school from æt. 2 to 16. Always weakly. Now phthisis. Memory good; remembers correctly some details about the school. May have had a bad attack when very young, but if so it would probably have lasted long enough for him to remember something about it. Probably a case of pure uncomplicated granular ophthalmia of considerable severity which never produced subjective symptoms.

‡ The entropion is worse in the lid which shows no scar.

In granular ophthalmia, more than in many other diseases, it is necessary to be very cautious in giving a prognosis from a single examination of the patient. The relation between the organic changes and the symptoms is so variable in different persons that a mere view of the conjunctiva, however careful and minute, will form a very incomplete basis for an opinion of the patient's future prospects. The following points will be of much service:—1. The approximate duration of the disease, if it can be ascertained; the future risk of serious symptoms or secondary structural changes will be small in proportion to the length of time that has already elapsed without them. 2. The present existence of subjective symptoms; these, if present, damage the prognosis, since the liability to them (*cæt. par.*) becomes greater after they have once appeared than it was before their first occurrence. 3. The same rule applies to corneal opacities and ulcerations; a large majority of ulcerations occur as relapses in patients who have already suffered from them. 4. The presence of ophthalmia tarsi (sycosis tarsi) in addition to granular conjunctiva increases the risk of corneal ulceration, as well as the liability to alterations in the curvature, &c., of the lids and lachrymal puncta.

*Causes of granular ophthalmia.*—Although this disease has long been known and more or less perfectly described, no great care seems to have been bestowed on its etiology until it began to be recognised as forming a very important element in the epidemic ophthalmia which has occurred in most of the European armies since the year 1800, and which has been a serious disease in Egypt and some other hot countries for centuries.

Granular disease, as distinguished from contagious ophthalmia, was well described as a prevalent one among large collections of men by O'Halloran, in 1824. In speaking of the common error of comparing the "granulations" of this disease to those of an ulcerated surface, he says, ". . . . we see in the eyelids of persons who never suffered from the disease (*i. e.* purulent ophthalmia) an appearance similar to what is commonly designated 'granular.'" He goes on to say that this may be verified in "any body of men, viz. a military corps or regiment." And again, "In every large body of men persons are to be found whose eyelids are overspread with villous flocculencies, or fungous productions analogous to what has been denominated 'granulations,' notwithstanding that from youth they may have enjoyed health or absolute immunity from the affection under notice." After describing with some accuracy the chief varieties in the appearance of granular lids, he has the following passage as to the nature of the disease. The granulations are "simply enlargements of a fungous nature from the glands and vessels; the former, if I mistake not, being far more numerous than has been supposed;<sup>1</sup> that similar enlargements take place in scrofu-

<sup>1</sup> He does not seem to know that J. B. Müller had, in 1821, concluded that con-



lous habits where the glands in other parts are diseased; that in such cases the affection of the glands of the eyelids is sometimes the cause, and not the effect, of what is called inflammation." The full importance of follicular or "vesicular" granulations as predisposing to severe attacks of conjunctivitis was not pointed out till some years later. They were accurately figured by Eble in 1828<sup>1</sup> and described by him as forming the first stage of contagious ophthalmia, and in 1839 Hairion gave them an important position in the disease. The fact, now universally admitted, that they especially predispose to attacks of severe ophthalmia, was observed by Loëffler in 1848, and has since then been abundantly confirmed.

The gradual discovery of the important rôle played by these granulations in the destructive ophthalmia of European armies led to the assumption in the first place that they, as forming one of the symptoms or results of the so-called Egyptian ophthalmia, had been imported into Europe from that country about 1800. When it was at length discovered that they are not the result of purulent or other acute forms of conjunctivitis it was asserted by many authors that two distinct diseases of the conjunctiva had been imported into Europe from Egypt—the vesicular granulation, or "vesicular exanthem of the conjunctiva," and purulent ophthalmia. With regard to various forms of acute conjunctivitis, it was allowed that they had been known to occur both sporadically and epidemically in Europe long before the Anglo-French Egyptian campaign; but it was contended that granular ophthalmia proper, not having been described as a prevalent disease, could not have existed in Europe (or at most only sporadically) until introduced from Egypt, and that, in fact, it had been so introduced. Inasmuch as the true nature and importance of vesicular granulations was not discovered until long after the alleged importation of the disease from Egypt, it was thought by some that descriptions of similar appearances by early authors could not safely be taken as referring to the disease in question. Further, it was held that the negative fact of these granulations not having been noticed by modern observers in a given country until the occurrence of severe ophthalmia in the army or civil population of that country drew attention to the subject, was equivalent to proof that they had no existence until they were seen. That granular ophthalmia was introduced from a distant part and afterwards spread very widely among the military and civil populations of almost every European country, necessitated the doctrine of contagion, and we accordingly find that several authors adopted, with some modifications, Thiry's hypothesis of a special

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tagious ophthalmia was "a disease of the mucous glands of the eyelids." *Congrès d'Ophthalmologie*, 1857, 'Compte-Rendu,' p. 291. I have not yet been able to examine the treatise of 1821 here referred to.

<sup>1</sup> These are the best illustrations I have seen.

“granulous virus.” This was supposed to be generated in the vesicular granulations and thence to escape into the air in a “gaseous state” after their rupture (Landau), and to propagate itself both by direct aerial infection and by contaminating bedding, clothes, &c., from which it might again be given off in an active state after as long a period as three years (Hairion). It was further asserted that the disease never originated *de novo* in Europe, its origin in this way being confined to Egypt, and, perhaps, some other hot climates. To this a few authors added the still more startling proposition that it was a constitutional blood-disease.

Many writers have now abandoned the theory that contagion is the cause of follicular granulations. The expressions in most of the text-books are now to the effect that granular ophthalmia is contagious when accompanied by and in proportion to the amount and purulence of the discharge.

Although evidence that granular ophthalmia existed in Europe before the date referred to would not weaken the hypothesis that it is contagious, this would be the effect of evidence showing that other more probable hypotheses would account for the facts equally well without recourse being had to a supposition which is quite without parallel in pathology.<sup>1</sup>

It is difficult to understand how the evidence that this disease was present in Europe prior to 1800 can have been disallowed by any one. Many authors have asserted it and quoted early writers in support of their opinions, among the most recent of these being Welch in his very able and valuable paper in 1869. As there is still some want of conviction on the subject however, it may not be amiss to repeat some of the best known evidence, much of which has been quoted in a more or less scattered form by various authors.

The accurate description of the various kinds of trachoma given by Aetius, including an account of one kind which can refer to no other disease than vesicular granulations, I omit, as it is probable that his experience was drawn largely from oriental countries where, as in Egypt, it is admitted by most authors that the disease has existed for centuries. Scheffer’s brief description (1678) of an ophthalmia in Lapland can scarcely refer to any other disease, since it may be safely asserted that granular ophthalmia is an essential part of any permanently endemic eye disease which destroys or seriously injures the sight of a large number of persons in a given

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<sup>1</sup> The hypothesis (unconfirmed by demonstration, as in ringworm, &c.) is that a contagium is given off by a very small part of the mucous surface of the body in a state of extremely chronic and purely local disease, of so volatile, active and long-lived a nature as to be capable of infecting the great majority of those exposed to it, and of being transmitted, after long preservation, through the medium of infected objects.



country or locality. He says, "Diseases of the eyes, which they inherit in an inflamed, watery and bleary condition, is their commonest malady, and is very often followed by loss of sight. The smoke with which their cabins are always full, summer and winter, and the light of the fire, in the front of which they sit all their lives, cause this disease." The next instance, occurring in Ireland, though more detailed and circumstantial, may be objected to on the ground that a mere acute purulent ophthalmia would have caused the same results. This is, however (as in the case above quoted), so unlikely in a civil population living under their ordinary conditions, that it may be put aside. It was doubtless an epidemic of purulent ophthalmia in persons who were already the subjects of granular disease. The instance referred to is mentioned as follows by Mr. (now Sir) W. R. Wilde:—"So long ago as 1701 an epidemic of this nature (*i. e.* destructive ophthalmia) broke out near Castle-town-delvin, in the county of Westmeath, by which vast numbers lost their eyesight." It is much to be regretted that the original manuscript by Sir Thos. Molyneux should not have been published under Mr. Wilde's direction; it is entitled, 'Queries proposed by Dr. T. Molyneux to John Hill, curate of Castle-town-delvin, in the county of Westmeath, concerning the extraordinary distemper that took away the sight of many in that parish.' It occurred, like many subsequent epidemics, during a time of general distress in Ireland, during which many thousand woollen manufacturers had to emigrate for want of employment, and "the whole kingdom was reduced to the greatest poverty and distress." Sir Wm. Read was evidently acquainted (about 1710) with various kinds of granular lids, including primitive vesicular granulations, eyes so affected, showing "an inequality and roughness of both the eyelids in their inward part, with an hard ruggedness, as if the seeds of millet were within them." The last comparison would be still more valuable if it were not identical with that used by Aetius. Maitre-Jean, in 1740, described granulations on the lids; and a year later St. Yves, writing on 'Tetter of the Eyelids,' says, "if the *eyelids* be turned out they appear red in their *inside*, and seem to have *inequalities* somewhat like the *small grains* of figs."<sup>1</sup> In his inaugural dissertation (1751) Koenigsdoeffler describes soft, yielding, but solid, prominent "tubercles" on the inner surface of the eyelids. He adds, in order to distinguish them from the villous lid and the harder granular lid of Aetius, that in the disease he was describing "there was no roughness of the lids." He gives (p. 10) a case of general scrofula in which the eyes had been inflamed for two years, and "the morbid matter of the disease having gone to the eye, had not only affected the Meibomian glands but had raised tubercles on the inner mem-

The word "sycosis" was used by the very early writers to designate advanced and prominent granulations on the conjunctiva.

brane of the eyelids, which appeared to increase the inflammation of both eyes excessively." The glands in the palpebral conjunctiva (which he carefully distinguishes from the Meibomian glands), are, he says, more easily seen in the lower animals than in men, are found all over the upper lid and on a part of the lower lid, "consist of very close and minute lenticular particles, varying in size and shape," and they "secrete serous and not sebaceous matter." It is extremely probable that granular disease existed among the horses of Ireland more than a century ago. In the winter of 1750-51 there was in Ireland "a universal catarrhal fever among horses;" "and many of the labouring horses who had this disorder suffered so much in their eyes as to have become blind." The same thing occurred again in 1760, and, writing two years later, Rutty says it was computed that one horse in every ten affected had been permanently blinded. Lymphatic follicles are known to exist in the normal conjunctiva of the horse. The 'Trachoma herpeticum' of Plenck (1783) clearly refers to vesicular granulations.

The above extracts appear to show that granular ophthalmia in all stages and varieties was well known in several widely separated European countries between one and two hundred years ago, and that consequently it does not own an Egyptian origin. Whether it was relatively as common then as now it is impossible to say, but it would probably not be so if the best supported theories of its cause are true. The negative fact that it was not known to be prevalent is of no value considering the state of medical knowledge at that time. Nor can I attach any great importance to assertions that it was unknown in Strasburg till 1851 (Stoeber), in Belgium till 1814 (Hairion), in Algeria till 1859 (Hairion, Furnari), in Denmark till 1848-51 (Bendz), in Russia till 1816 (Kabath), &c. We are nowhere told that vesicular granulations had been carefully looked for and found absent, but only that they had not been observed until a severe outbreak of contagious ophthalmia drew attention to the state of the conjunctiva in those who were supposed to be healthy. In the case of Algeria, moreover, statements differ, and even the same author gives evidence in one part of his works which is inconsistent with assertions in another. Thus Furnari, while denying its existence in Algeria in 1859, describes sequelæ of ophthalmia (granulations, pannus, entropion) in that country in 1844 which leave little doubt that the true granular disease was at the latter date far from rare. Cuignet, in his recent book, maintains the identity of Algerian and Egyptian ophthalmia and believes that it was well known in Algeria long before the French conquest, which was, according to Hairion and Furnari, indirectly the means of its introduction into that country by way of Europe.

With respect to the contagious character of granular ophthalmia



there are various shades of opinion. Some observers hold that not only are follicular granulations caused by contagion and in no other way, but that the contagium is not of necessity associated with any perceptible conjunctival discharge, is given off by the simple sago-grain granulations, becomes volatilised and acts at a distance, and may have a long period of latency in the conjunctiva. To this a few add that the virus may be absorbed by the pulmonary mucous membrane as well as by the conjunctiva, and that the disease is a constitutional one. Others, who do not go to this length, believe that though the granulations are caused by contagion, the contagium does not exist or cannot act unless there is conjunctival discharge, and that this is indistinguishable from the product of an ordinary conjunctivitis (*i. e.* inflammation of a non-granular conjunctiva), except by its specific power of reproducing vesicular granulations. According to a third school, contagion is only one out of several causes of granular disease, and also requires purulent or mucopurulent discharge as its condition of action. A fourth group maintains that contagion has no part in the matter, that follicular granulations are due to various faults in the conditions of life and will always be reproduced when those conditions are allowed to come into play. De Condé and Schmidt, in 1841, held that granular ophthalmia should be compared to typhus in being a blood disease of which one attack was protective against a second, and attempted without much success to support this position by statistical and clinical evidence intended to show an antagonism between this disease and typhus, dysentery and intermittent fever. Many others, such as Müller, Bendz and Hairion,<sup>1</sup> held that it was a local specific disease propagated by aerial infection, with a period of latency during which the granulations, though invisible to the naked eye, might sometimes be detected by a lens and were at any rate liable to grow and become active on very slight provocation; and that "it is transmitted without the conjunctiva being the seat of any visible morbid secretion, solely by contagion, and generally by miasmatic means" (Bendz), by the agency of a special "granulous" or "vesiculous virus." Marston and Frank, though they do not express themselves in so unreserved a manner, appear to hold very much the same opinion, as also do the French editors of Mackenzie's treatise. Thiry may be taken as the leading exponent of the second school. According to him the "granulous virus" is the product of a peculiar, though acute and virulent, inflammatory process, and has for its result the formation of granulations and reproduction of the same specific purulent discharge. He considers that true granular ophthalmia is the result of an acute process, and does not admit "vesicular" and "fleshy" chronic granulations within the pale of

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<sup>1</sup> Dr. Hairion was still of the same opinion in 1873.

the true disease. Wecker represents the third school when he states that granulations are contagious because of the purulent secretion accompanying them and in proportion as this approaches the characters of pure pus, while granulations unaccompanied by discharge are but rarely contagious. An almost identical opinion is expressed by Stellwag, and other recent writers appear to take nearly the same view. The ordinary cause of chronic granulations Wecker places in prolonged exposure to air rendered impure by overcrowding, though he does not specify which constituent, if any in particular, of this impurity is the actual cause. Soelberg Wells is still rather ambiguous, and "has little doubt that vesicular granulations are contagious," with the qualification that this is especially true when they are accompanied by swelling of the conjunctiva and by a little muco-purulent discharge. Overcrowding and prolonged exposure to impure air, to which some add exposure to smoke and the exhalations of domestic animals, are mentioned as the cause, with more or less clearness, by Rust, Scheffer and many other early authors, and more recently by Anagnostakis (1857) and Schweigger (1870), and by many of the authors who believe that contagion is in part a cause. The view that contagion plays no part at all in the causation of follicular granulations was more distinctly put forward by Welch in 1863 and again in 1869. He concludes against contagion on account of the almost invariable symmetry of the disease and because it does not spread among a body of men in any relation to the facilities for direct contact. The argument from symmetry would be worse than worthless if directed against infection through the air at a distance or by fomites, but is of some value as against direct contagion. The second reason is a very potent one. He gives much evidence in favour of the intimate relation between nocturnal impurity of air in dormitories and the appearance of vesicular granulations, and thinks it probable that living habitually in an atmosphere contaminated by organic matter (not, as he is careful to say, epithelial or pus-cells) is the actual cause, but that warmth and moisture of the air are essential conditions of its action. Dr. Leith Adams also concludes in favour of crowding and bad ventilation, and mentions habitually impure air in bed-rooms as a possible cause of the disease among persons living under apparently good hygienic conditions. Brudenell Carter suggests that the decomposing organic matter contained in fustian may have something to do with the greater prevalence of the disease among the boys than the girls in schools. Many firm contagionists are also believers in the great importance of anti-hygienic conditions, especially Marston, whose conclusion "that the state of the conjunctiva offers a delicate test of the hygienic state of a regiment" has almost passed into a formula.

Welch's hypothesis, supported as it is by many careful observa-



tions, that excessive moisture of the air is essential for the production of vesicular granulations, if it be not the cause itself, appears to me a very important one, more especially as it bears out the opinion so very often expressed by authors, that moisture is one cause and an important one, of various forms of epidemic ophthalmia. I shall therefore go somewhat into detail in the quotation of opinions and facts bearing on this subject, keeping to such as refer to or at least include in their account granular ophthalmia, as well as the forms of conjunctivitis proper.

One or two negative facts may be conveniently mentioned first. Such negative evidence is not altogether free from fallacy, since in a country where the soil and air are generally very dry the population is almost sure to be thin, and thus there will be an absence of other probable causes besides damp. In Egypt, Power and Anagnostakis tell us that the Arabs of the Desert parts, "where the heat is intense but the air very dry," do not suffer from ophthalmia; and Burton says the same with respect to the inhabitants of Western Arabia. "Every one knows," he says, "that ophthalmia is unknown in the Desert, and the people of El Hajez, who live in an atmosphere of blaze and sand, seldom lose their sight." Ophthalmia is well known to be very common and severe in Algeria, but Lustreman tells us that the inhabitants of the mountains and sandy plains of that country are free from it.

The evidence as to the effect of moisture (both of air and soil) may be divided into two parts. First, there are the general facts as to the prevalence of the disease in countries which are habitually moist and in damp districts or towns in otherwise dry countries; and secondly, some more precise statements as to the observed effects of moist or dry climates or localities on the conjunctiva of persons living there. The second part is more scanty, and perhaps not always so reliable as the first. Evidence of the first kind is of course not free from fallacies; nor do I wish to attempt anything like a complete exposition of all the facts that might be collected but only to mention some well-marked instances pointing in this direction. The two most important fallacies are, that the largest and most crowded cities are usually built on low and more or less damp sites, and that acute catarrhal forms of conjunctivitis are also extremely common in damp changeable climates and sites, so that it is often impossible to distinguish, from a written account, how much of a prevalent ophthalmia is due to them and how much to granular disease.

A very striking instance of a habitually damp country where granular ophthalmia is known to be very prevalent is Ireland. It is far commoner among the Irish than either English or Scotch, so much so that the tendency to it is not only permanent in natives of Ireland wherever they live, but is to a great extent hereditary, the

children of Irish parents being much more subject to it than those of some other races, even though they do not live in Ireland.<sup>1</sup> Egypt furnishes another equally prominent and instructive instance, although less simple, the element of moisture being there combined with other agencies which are more or less prejudicial to the conjunctiva, especially dust and intense light and heat. It is the case, however, that in Egypt ophthalmia (of which the granular disease is as important an element as in the ophthalmia of Ireland) is most abundant in the dampest parts of the country. According to Dewar and Power almost the only parts of Egypt which remain wet all the year round are at and near the rice-grounds; and the latter author tells us that "those employed in the cultivation of rice are peculiarly affected with blindness." The soldiers of the French Army suffered less from the disease in Upper Egypt in the dry season than those in Lower Egypt (Dewar). The "warm and moist climate of Egypt" is considered by Landau to be the birth-place of the "vesiculous virus." According to Anagnostakis the atmosphere of Lower Egypt during the flood period of the Nile is extremely damp, and in Alexandria, he says, this dampness prevails from the end of spring till November.<sup>2</sup> Scoresby Jackson makes statements of a similar general character with respect to the atmosphere of Lower Egypt, and especially of Alexandria, as compared with that of Middle and Upper Egypt. In Algeria "low and damp country" is specified by Cuignet as one of the most powerful predisposing causes of ophthalmia. Furnari describes the dwellings of some of the Arabs in Algeria as "swimming in mud" in the winter. Laveran says that purulent ophthalmia in this country affects particularly the "planters of the low unhealthy villages, the Arabs of the towns, and especially the inhabitants of the oases crowded together in infected huts built on a wet soil under the palm trees." According to Lustreman those Arabs of Algeria "who live in the damp coast stations, or in huts surrounded by water in the oases, show all the ocular changes which have gained for Cairo the title of the city of the blind." In Malta, where granular ophthalmia has since the beginning of this century (and probably long before) been very prevalent, Hennen says that the air in autumn is excessively moist and hot, and that the island is by no means free from marshy districts, between two of which Valetta is situated. Suez, where ophthalmia is sometimes very destructive, is also subject to inter-

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<sup>1</sup> I first learnt these facts from Mr. Hutchinson.

<sup>2</sup> The Nile begins to overflow its banks in Egypt about two months after the summer solstice, *i.e.* about the end of the third week in August. The inundation continues for two or three months, more or less, and its bad effects have ceased by the end of November. — 'Encyclop. Brit.,' Article "Egypt." Scoresby Jackson, 'Medical Climatology.'



mittent fevers and is situated on or close to a large swampy surface of seashore which is left bare at certain seasons. Florio is emphatic on the efficacy of damp air and buildings as one of the most important causes of the disease. The coincidence of prevalent and prolonged ophthalmia among soldiers with an especially marshy and foggy site on the plain of Marengo, where intermittents and dysentery were common, is mentioned by Carron du Villards.

Of more precise statements we have the recent and valuable observations of Welch to the effect that in a given regiment, while living at Malta in barracks where a high rate of aerial moisture was, among other bad conditions, pretty constant, follicular granulations increased vastly; although even here they varied at different times according to the quality of the barracks occupied and were, as a rule, worst in those "companies" which occupied the worst ventilated and dampest quarters. When the same men were moved to New Brunswick where, among other things, the air was on the average much drier than at Malta, the disease receded in those previously affected and did not occur in any who had been free from it in Malta. The same was the case also with a number of men who were sent from the home *dépôt* to join the regiment in New Brunswick; their lids, which on their arrival in Canada showed nearly half to be affected, very much improved while there. The first recorded severe outbreak of ophthalmia in Portugal was in a large school of several hundred boys and girls who lived in a dark and damp underground building. Dr. Massy in 1851, says of Athlone (in county Westmeath, Ireland), that "the atmosphere is perhaps the dampest that I have ever experienced, except, perhaps, in India, during the rains;" that it "is well known as a locality where ophthalmia constantly prevails," and that almost every regiment quartered there suffers from it. The 31st Regiment, of which he is writing, had suffered slightly from the disease before being quartered at Athlone in 1848, but after being there nearly twelve months the disease became much more prevalent among the men, and during the next year some very severe cases occurred. Hennen gives interesting details as to the effect, in Malta, of moist air in aggravating half-cured cases of military ophthalmia. In 1807 Mr. Vetch drew up a special report on the effect of moisture on ophthalmia, in which he gives many interesting details of the bad influence of damp localities and foggy wet weather on cases of military ophthalmia in Kent, and the benefit of drier situations and dry weather in favouring the recovery both of chronic and recent cases. In his opinion the hottest weather is best for convalescents, *if clear and sunny*. He thinks the greater moisture of England accounts for the disease having been more violent and destructive in the Army in this country than at Gibraltar, and, as he thinks, even than in Egypt.

In the case of the Metropolitan pauper schools, it is worth while to remember that their inmates have in the majority of cases not only lived in much overcrowded houses, but are also natives of the Thames valley. It is true that several of the schools which have suffered most from the disease are situated on a level much above the Thames, but there may be other facts which would diminish the value of this objection. It is not impossible that a difference of climatic aerial moisture may have some share in the absence of troublesome ophthalmia from many pauper schools in the country districts. Excess of moisture is, under ordinary conditions, a constant constituent of air rendered impure by animal respiration, so that it is quite possible for persons who spend a large part of their lives in ill-ventilated rooms to be thus exposed to the effects of moist air, even though the climate of the country or district may be dry. It would be worth inquiring whether persons (especially children) who work in hot, dry rooms where the air is not often changed,<sup>1</sup> show a smaller average of follicular granulations than others living in the same place but employed in other ways.

Whether the essential cause of follicular hypertrophy be moisture, either of air or soil or both, or some form of organic matter as Dr. Welch and many others suppose more likely, there can I think, be little doubt that moisture is an important condition in the development of the disease, and that in all probability it would be practically exterminated if the air habitually breathed could be kept dry enough.

It is of practical importance, even at the present time to determine whether pure granular ophthalmia is contagious or not, and to my mind the evidence is quite conclusive against this view. All the known facts go to prove that, whatever may be the cause, lengthened exposure to it is requisite (probably of many months). Welch thinks twelve months necessary. Massy's statement as to soldiers getting ophthalmia after nearly a year's residence at Athlone, though less definite, would appear likely to point to the same fact. Wecker thinks exposure for a *long time* to air made impure by overcrowding is one cause of chronic granular ophthalmia (he however, like Müller and especially Thiry, attempts to draw a distinction between granulations proper to contagious ophthalmia, and those elevations or "vesicular granulations" due to simple hypertrophy of the normal lymphatic follicles and mucous glands of the conjunctiva, a distinction quite impossible in practice, as he afterwards, in fact, admits). Gulz specifies "about one year" of exposure to great "overcrowding only" as necessary in order to cause granulations in a number of robust Austrian countrymen previously free from them. Gradman, with less precision, gives two months of garrison

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<sup>1</sup> I am told that such conditions can be found in cotton factories.



life in Denmark as generally giving rise to the early stage of the disease in about half the men. I am aware that some authors (especially Hairion) give instances in which a short exposure of healthy men to supposed sources of contagion, either by other men or by fomites, has caused the disease. The value of such observations is, however, much diminished by two facts. Most authors (Hairion prominently) believe that the disease may be latent for a certain time, during which it cannot be detected, or, at least, only with the aid of a lens; it is most unlikely that such a lengthened and elaborate inspection could be made in the case of any large body of men at one time, and so some men with "latent granulations" might easily escape detection. Secondly, any one who has examined a large number of persons for early granulations must, I think, have been puzzled sometimes to know whether on a given conjunctiva there were granulations or not, and it is no reproach to the care or skill of the observer to say that very slight cases (and it is to such only that the present observations of the contagionists refer) may be passed as healthy one day and as diseased a few days later, without any material change having occurred in the interval.<sup>1</sup>

If granular ophthalmia, while free from muco-purulent discharge, were contagious at all, it must be so by means of a very volatile and yet very powerful *contagium*, since so many of those who live permanently together suffer from it. But if the *materies morbi* were of this subtle and volatile nature, it would be most unlikely that other persons who, though not living with the diseased ones, come into frequent and prolonged relations with them, would escape. We are told, however, that military officers are seldom affected by the disease though they are known to suffer from the (hypothetically) much less contagious purulent ophthalmia. From the nature of the case it is, however, impossible in the present state of knowledge, to look upon the hypothesis of contagion as absolutely disproved; indeed, the virus is supposed to be so subtle, so powerful and so longlived, that it would be extremely difficult to

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<sup>1</sup> The chief difficulties in examining the lower lids for granulations are found in thoroughly exposing the oculo-palpebral fold of the lower lid, and in the congestion which comes on almost immediately after the lid is everted. Without exposing the oculo-palpebral fold at the outer part of the lower lid it is impossible to be certain that there are no follicular granulations, since they almost always begin in this part. If the patient will not look up enough to bring the part well into view when the lid is drawn down, or if from prominence of the orbital rim or tightness of the skin no effort will suffice, slight pressure on the eyeball through the upper lid will generally be successful in making the fold start forwards. The second difficulty can only be partially avoided even by practice in quick observation and gentle handling; when every care is taken it often happens (as Frank has observed) that the congestion obscures even very obvious granulations (they almost disappear as one looks at them), and renders the detection of incipient ones very difficult.

devise experiments showing the spontaneous production of the disease which would not be rejected as fallacious by a thorough-going contagionist such as Dr. Hairion. It can only be shown to be extremely unlikely.

Among predisposing causes the general health of the children will have an influence on the development of sago-grains. Allowance must also be made for inherited tendency to the disease in children of Irish race (and, according to Scheffer and Burton, for those of Lap and Egyptian parentage respectively), and for original differences of tissue, by which, indeed, inherited tendency is probably expressed. The proportion of Irish or half Irish children is probably much larger in the Metropolitan pauper schools than in those of any other part of this country (with a few well-marked exceptions, such as Liverpool, perhaps). This, no doubt, contributes to the frequency of the disease in the London schools. Some of the worst cases of pure granular conjunctiva that I have seen in children have been in those who had thick, fleshy, red lips, thick skin with abundant subcutaneous areolar tissue, and coarse, straight, dark hair. Thick fleshy lips, I think, more frequently accompany it than the thick skin and coarse hair. Then the tendency to inflammation of the eyelash follicles and Meibomian glands, which often precedes severe forms of granular lid with great papillary hypertrophy of the tarsal part of the conjunctiva and considerable corneal damage, often runs in families, the special details of each case being almost exactly the same in several brothers and sisters. I have several times noticed that children whose skin is fine in texture, thin and fair, and whose hair is generally sandy or red, show thin conjunctivæ without any great development of sago-grains. These differences in disease no doubt correspond to similar variations in health. Welch has noticed that men with fair hair and ruddy skin sometimes have less than the normal vascularity of conjunctiva. I have several times noticed the same thing in healthy children, but am inclined to think it is generally seen in those whose skin is thin, who have little cellulose-adipose tissue, and *bright* coloured hair. Very fair hair, or light hair of a dull tint with thick lips and perhaps "fleshy" skin, not uncommonly accompanies bad granular disease with considerable implication of the tissues at the edge of the lid (sycosis tarsi).

(To be continued.)



## II.—The Transmissibility of Tuberculosis.

By GEORGE FLEMING. M.R.C.V.S., F.R.G.S., Royal Engineers.

ONE of the most interesting and important, and, indeed, it would appear urgent, questions of recent times, as far as pathology, human and comparative, as well as sanitary science, are concerned, is that of the probable, nay certain, transmission of tuberculosis from mankind to animals, and possibly from these to the human species or to each other. The infectiousness or contagiousness of this morbid state is certainly no recent belief. Morgagni, many years ago, asserted that phthisis was communicable from one person to another, and a few physicians have now and again since then, though somewhat hesitatingly, it is true, expressed their conviction that it was more or less capable of transmission by cohabitation; and, though rarely, stress has been laid upon the great danger of allowing healthy people, especially young children, to associate with those who are consumptive.

Notwithstanding all this, however, tuberculosis has not been generally recognised as contagious in the human species, and, indeed, it has been denied that it possesses this property.

Though the disease is one of the most prevalent and fatal of any affecting mankind, it is far from being uncommon among animals, and especially those kept in confinement, as in menageries and zoological gardens; while among those which are domesticated, it is of somewhat frequent occurrence. Of the latter, the bovine species suffer far more than any other; and it is this fact which invests the question of transmission with such supreme importance, and causes us to look forward with so much anxiety for the solution of the serious problems involved in it. The flesh of the ox is consumed as food, and it is essential that it should be in as healthy a state as possible; though the process of cooking might be relied upon, to some extent, to annul any pernicious properties it might have acquired through the animal having been in a diseased condition during life. Far otherwise is it with the milk derived from the cow. This seldom undergoes any process which would be likely to destroy whatever injurious qualities it might chance to be endowed with through the unsanitary state of the animal which yields it; and when we reflect that milk enters largely into the diet of young people—is, indeed, the chief aliment of children and infants, whose bodies it builds up—the question of acquiring disease in this way, and especially such a malady as tuberculosis, becomes one of the gravest moment.

It has just been stated that the bovine species is far more extensively affected than the other domesticated animals; indeed, there appears to be a strong predisposition to tuberculosis in cattle, though from an absence of all statistics with regard to diseases among

animals, we cannot arrive at anything like an approximate notion of the percentage affected with phthisis. There is no supervision of the slaughter-houses with regard to the state of the animals killed therein; and in private establishments, such as are now tolerated everywhere, such a desirable measure could scarcely be carried out in a satisfactory manner; so that we cannot hope to ascertain the extent to which the malady prevails, and must content ourselves with the fact derived from professional experience that a very large proportion of the cows which have been kept for any length of time in badly constructed sheds, and milked freely, are more or less diseased. For France, Cruzel says that in a hundred old oxen, fattened and slaughtered for food, at least one half will have the lungs more or less tuberculous; and in a hundred cows, those which are not are so affected in very small proportion. In many instances the amount of tubercular matter deposited in and around various organs is something astonishing: the heart and lungs of the cow in some cases weighing seventy, eighty, and even ninety pounds. The masses of tubercle may be deposited on the membrane lining the chest, and may vary in size from that of a mustard seed to that of a hazel nut; the lungs may also be more or less involved, as well as the spleen, liver, mesenteric glands, kidneys, superficial glands, peritoneum, and intestines. Sometimes immense irregular masses are found extending beneath the spine, from the anterior opening of the chest as far as the kidneys, often attaining their largest dimensions at the root of the lungs. At times, though rarely, tubercular deposits are met with in the muscles and other parts not specified above. Of course the butcher, who is well acquainted with the anatomical characters of the disease, takes good care to remove every trace of these suspicious-looking masses from the carcase of the animal in the process of dressing it for the stall; so that unless the inspector is more than usually alert and well qualified for his duty, such a carcase may easily escape detection. And even if the presence of tubercle be discovered, I am not aware that any suspicion of danger exists as to the utilisation of the flesh, or even of the tubercular matter, as food. It is a notorious fact that the milk of phthisical cattle is regularly sold for food, and that these creatures, even in the most extreme state of disease (in Scotland they are known to the butchers as "piners") are sent to the slaughter-house, whence their flesh is carried to the butcher's stall, to be disposed of to the poorer classes.

For many years the identity of this bovine disease with human phthisis was denied, and, indeed, a very recent observer, Professor Reynal, of the Alfort Veterinary School, yet maintains that the bovine and human tuberculosis are essentially different in their histological features. Describing the elements entering into the composition of the bovine tubercle, and pointing out that there are



no blood-vessels in its texture, this authority says, "The complete absence of nutritive vessels in the proper tissue of the tubercles explains why these, being nourished exclusively at their periphery, must necessarily only acquire a very small size before the central necrobiosis begins: a special peculiarity which distinguishes them from the sarcomatous tumours with which Virchow has unrestrictedly assimilated them. Also, notwithstanding the respect we entertain for the opinions of the learned histologist, we cannot altogether accept his notions of the subject. The elements of the bovine tubercle are identical with those of sarcomatous tumours: this is incontestible; but the absence of nutritive vessels in the first, and, from this circumstance, the determinate volume which they can never exceed, are two sufficient reasons, in our opinion, for separating it from the latter. Thus, for us, this new formation is neither the same as human tuberculosis, as the majority of writers would have it; neither is it a kind of sarcoma, as is affirmed on the other side of the Rhine. It is a typical lesion, having its own proper form and individuality, belonging exclusively to an animal species, and incapable of being transferred to another, notwithstanding what has been affirmed to the contrary."<sup>1</sup>

It must be observed, however, that Schüppel,<sup>2</sup> Professor of Pathological Anatomy and General Pathology at the University of Tübingen, who has made an almost exhaustive study of tubercle in general, and particularly that of the bovine species (tuberculosis in cattle being known in Germany as the "Pearl-disease"—*Perlsucht*, from the appearance of the tubercular masses, which are called "grapes" and "angle-berries" in this country), has fully established the absolute identity in structure and development of the cattle disease with tubercle in man. According to this authority, the first visible step in the formation of a tubercle is the presence in the interior of small blood-vessels (diameter of 0.02 to 0.03 millimètre) of peculiar masses of more or less globular form, made up of a congeries of dark granules, held together by a homogeneous connecting substance, and varying in size from 0.01 to 0.03 millimètre, having no nucleus nor limiting membrane, but offering a granular outline. These may occur simply in the blood-column, or two or three near together; they are most frequently met with in veins, more rarely in capillaries, and very seldom in arteries. Schüppel regarded them as masses of protoplasm, and proposed to call them, after Kölliker, "non-nucleated protoblasts." The walls of the vessel containing these show no alteration, but when they are large and block the tube, it usually has a fusiform dilatation. The first change noticed in the

<sup>1</sup> Reynal, 'Traité de la Police Sanitaire des Animaux Domestiques.' Paris, 1873, p. 701.

<sup>2</sup> "Ueber die Identität der Tuberculose mit der Perlsucht." Virchow's 'Archiv,' p. 38, 1872.

protoblast is in its most external layer, which loses its granular character and appears as a smooth limiting membrane or line. Nuclei soon show themselves in its substance, and thus it becomes a giant-cell (*Reisenzellen*), the most essential element of a tubercle, and which, when fully developed, may contain from one to eight or ten of these. How the nuclei are formed could not be ascertained, but it was believed that they arise independently of any pre-existing nuclei, like the nucleus of the first cleavage-cell of the ovum after the disappearance of the germinal vesicle. In some giant-cells, however, in addition to the perfectly formed nuclei, faint homogeneous bodies of smaller size are visible, and these were imagined to be an early stage of the former. When once formed the nuclei multiply by division, as may be concluded from their often presenting an hour-glass shape, and containing two nucleoli. The giant-cell now ceases to present its original globular form, becoming irregular in shape, and putting forth branched protoplasmic processes—changes due to its inherent vital contractility. No account could be given as to the condition of the vessel in which the protoblast was originally situated; as by the time the nuclei become developed, the vascular wall ceases to be visible. The epithelioid cells, as well as the reticulum, which is not present in the earliest stages of the tubercle, in all probability derive their origin from the original giant-cell. The protoplasmic processes of the latter often present, at a short distance from the parent cell, fusiform nucleated dilatations, and by the detachment of these new cells arise. The reticulum is also most probably formed by the processes sent off by the giant-cell; these branch and unite, and surround the first brood of new cells, and so form the meshes in which the other elements of the tubercle lie. The giant-cells appear at last exhausted, as it were, by this constant proliferation, and in older tubercles form shrunken branched bodies with few nuclei. The original protoblast was supposed to arise by free cell-formation: a process the possibility of which, though long strenuously denied, may be considered as probable, according to the researches of recent observers. Schüppel considers the proximate or exciting cause of the formation of primary tubercle in all organs to be a local inflammation, while the remote cause is the scrofulous or tuberculous diathesis, inherent or acquired; and though he admits that the tubercle detritus possesses infective properties, and is capable of causing the formation of tubercles both in its neighbourhood and, by absorption, in distant organs, he would even here ascribe, in many cases, the eruption of tubercles to a predisposition of the secondarily affected organs, rather than to mechanical conveyance to them of the infective substance; for a mere mechanical distribution by the blood of the tubercle virus is incapable of explaining the apparently capricious manner in which the tubercles become disseminated. Thus, the frequent occurrence of tubercle in the glands situated in



the lymphatic territory of tuberculous organs may be rather due to their being already the seat of simple irritative hyperplasia, than the conveyance to them, by the lymphatics, of any specific tubercular virus; as it has been shown that, in the lymphatic glands, tubercles never originate in the lymph-paths, but always in the follicles.<sup>1</sup>

So much for the identity and histological anatomy of tubercle in man and the ox. With regard to the causes of the malady in the latter, it has long been admitted that it is hereditary, and strong proofs are afforded in support of this. In the human species its transmission in this way has been traced by Lebert in one sixth of the cases investigated by him; but veterinarians have not yet afforded the statistical enumeration necessary to establish the extent to which it is thus due in the lower animals. Temperament and conformation, which are also hereditary, have for many years been considered to bear an important share in the appearance of tuberculosis in cattle: those of a lymphatic nature, with thick skins, flabby tissues, and light unusual colours, being most predisposed. Damp localities and dwellings, sheds badly ventilated and lighted and kept in a foul condition, sudden checks to the cutaneous transpiration (in draught oxen), and the mode of feeding as well as the quality of the food, have all been reported as favouring or actually causing the appearance of the malady in the bovine species. There can be no doubt that excessive and long-continued lactation has a powerful influence in the production of tubercle in cattle; as those which have been kept in dairies for any length of time are nearly always phthisical, and either die, or when sent to the butcher their carcase exhibits tubercles in various organs. Lafosse,<sup>1</sup> the eminent professor at the Toulouse Veterinary School, in commenting on this fact, states that the majority of the cows employed in agriculture—as they generally are in France, and particularly in Languedoc—as well as those kept for breeding purposes, and which are only required to give milk for six weeks or two months, most frequently reach the end of their career without being so affected. He also remarks that in mountainous districts, where the calves are suckled for five or six months before they are slaughtered, phthisis is more frequent.

It is this tendency of milch cows to become tuberculous which renders the question of transmissibility so serious and worthy of attention by the physician and sanitarian, no less than by the veterinarian, and calls for the closest observation and research.

The transmission of the malady by cohabitation has scarcely been suspected by veterinarians in this country; at least I am not aware that we have any evidence of this in the literature of British veterinary science. Several Continental veterinary surgeons have, however, insisted upon the contagiousness of the disease. Dupont, of

<sup>1</sup> Cayley, 'The Medical Record,' 1873, p. 49.

<sup>2</sup> Lafosse, 'Traité de Pathologie Vétérinaire,' vol. i, p. 432.

Bordeaux, for instance, some twenty years ago, stated his belief that such a mode of extension was possible; and Cruzel, an old and able practitioner at Grenade-sur-Garonne, in his recent excellent work on the diseases of cattle, does not appear to entertain any doubt in this respect. Speaking of the causes of the malady, he writes:

“Much has been stated with regard to the danger animals incur by being kept in low, narrow, and badly ventilated buildings, and this danger is real; it exists, and may be remarked every day. But all has not been said with regard to this matter when treating of tubercular phthisis. If in a cramped low stable, in which several milch cows are lodged, there chances to be one affected with this disease, and if the tubercles are in a state of ulceration and suppurating, so that the air expired from its lungs is very fetid; then this air, inspired immediately by another cow, carries into its healthy lungs the tubercular infection. It is indeed rare that the breath of animals of the bovine species affected with phthisis has not a remarkable odour *sui generis*; and for a long time, and when called upon as an expert to examine cattle suspected of being affected with phthisis, the first indication I have sought for, on which to base an opinion, has been the odour of the expired air. Whenever I perceived a well-marked and characteristic fœtor, I have diagnosed phthisis (*vulgo* ‘pommelière’ in France) as certainly present. An ox affected with cough, and therefore suspected, was sold, but had to be taken back by the vendor on his being threatened with legal proceedings. It was a fine animal of the Gascony breed, and appeared to be otherwise in excellent health. Its fellow (in draught), of the same breed, and also as well proportioned, lost condition, and its appetite failed, though it never coughed. Fearing that it might die, the owner determined to send them both to market, and get rid of them. He did so, and both were sold. Soon after the new purchaser made verbal but friendly complaints, not with respect to the lean animal, but the finest one, which had the dry, short, and frequent cough. The dispute was arranged to be settled by arbitration, and I was entrusted with the task of terminating it in an amicable manner. I consequently most carefully examined the ox whose condition was debatable, testing it while at work, by running, and at rest, auscultation being resorted to. From this examination I could not finally decide that the animal was phthisical; but I had several times inhaled the air issuing from its lungs, and from the fetid odour it possessed I was able to assert that there were ulcerated tubercles (*tubercles ulcérés*) in the lungs. Desirous of assuring myself of this fact by a necroscopical inspection, the animal was sent to the butcher; and I found in its lungs, which were healthy nearly throughout, a single group of tubercles about the size of a small nut. These were in different stages of alteration, and there was an ulcer with a callous margin, from which flowed a small quantity of sanies that had a foul



smell, quite the same as that which had attracted my attention and that of the assistants during my examination. It is in this way that tuberculosis is communicated by the expired air; and the facts which support this opinion are numerous. Those veterinary surgeons who practise in the country may gather them every day, and I may more particularly call their attention to the following circumstance:—Two working oxen or cows are brought into the same building, and they eat their forage or ration, or whatever it may be, out of the same rack or manger; lying in the same stable, they respire nose to nose. One is perfectly healthy, at least so far as appearances go; so is the other, except that it coughs from time to time, and its breath is fetid. Soon it is perceived that the other animal, which does not cough, and whose expirations are not fetid, if at first it ate with appetite, shows less desire for food out of the usual receptacle, hangs back from it, and neglects it; and to make it approach the rack or manger one is obliged to push it, to excite it with the hand, and even to beat it with a stick. Nevertheless, this animal does not appear to be affected with disease, nor even to be indisposed; and if it refuses to remain on the same spot as its comrade, it is because it is desirous of getting away from its fetid breath. The cowmen can scarcely be deceived; and if by chance there is an opportunity for selling the diseased animal, or if it can be fattened for slaughter, and its companion gets another healthy comrade like itself, then its appetite quickly returns and no further inconvenience is observed. But if, on the contrary, the situation is not altered by the separation of the two animals, phthisis runs its ordinary course in the one affected, and the other, at first becoming emaciated, is finally attacked with the same disease.

“After this, if we do not wish to recognise in such a case the effects of contagion, we may attribute them to cohabitation, infection, or anything we like; but we cannot hinder the fact from being present, nevertheless, and to see it reproduced very frequently in the circumstances I have just alluded to.”

Other French veterinarians have, since the publication of Cruzel's work, more distinctly alluded to the contagiousness of tuberculosis in the bovine species, and furnished facts which can scarcely be disputed.

Visieur,<sup>1</sup> of Arras, in 1868, endeavoured to explain the persistence of the malady in certain establishments, well kept in every respect, but in which the cattle were slowly decimated, and in which hereditary transmission could not be included as a cause of the mortality, as breeding was not carried on in them. In some of these places the malady had lingered for years, and had affected animals of all ages; but not until after they had been a certain time in the sheds, and in contact with the *vaches lactières poitrinaires*.

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<sup>1</sup> ‘Recueil de Méd. Vétérinaire,’ 1873, p. 881.

Zundel,<sup>1</sup> one of the most talented of the Continental veterinarians, has also commented on the frequent persistence of bovine phthisis in Alsace, in stables well kept and carefully ventilated; and this persistency, he is inclined to believe, can only be attributed to contagion. He alludes to certain establishments in the vicinity of Mulhouse, where, notwithstanding the greatest care in purchasing, and good hygiene and regular feeding, the healthiest animals become affected with the disease after dwelling for a certain time in the stables. Most frequently these are the strong and vigorous Swiss cattle, which, in the words of Zundel, "do not pay their tribute to acclimatisation, as certain people appear disposed to believe, but are in these stables under the influence of a contagion that the owners cannot comprehend, and look upon as a fatality to which they must submit." And yet other animals from the same country, and kept in sheds often less attended to, remain healthy for years. "Is not this a proof," he adds, "that acclimatisation goes for nothing in the mortality attending the first, and that it must be attributed to some circumstance inherent in the places where these accidents occur?"

The evidence of M. Grad,<sup>2</sup> veterinary surgeon at Wasselonne, Alsace, is still more significant and decisive, as it not only furnishes us with clinical observations, but also an experimental fact which adds a great value to the others, while it receives additional weight from them. The observations are guaranteed as perfectly authentic, and were collected during a period of twenty-three years' practice as a veterinary surgeon. At the commencement of his professional career, Grad does not appear to have believed in the possibility of tuberculosis being transmissible amongst bovine animals by cohabitation, but the close observance of a series of facts at length confirmed him in the opinion that it was. In order that the circumstances in which the transmission of the disease was most frequently noticed might be understood, this veterinarian describes the arrangement of the cattle-sheds of the farmers; and it would appear that the animals are generally kept in very short stalls, which completely separate the animals from each other, from the rack and manger to one third the width of the shed. This arrangement, while preventing the cattle from injuring each other with their horns, as they cannot reach one another with their heads, also permits their allowance of food to be given separately. On different occasions, owners had informed Grad that they had lost two, three, and four animals *in the same stall* from consumption—marasmus accompanied by cough. At first he did not attach any importance to these declarations, either because he thought the mortality occurring in this way was merely accidental, or was due to hereditariness, which he acknowledges plays

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<sup>1</sup> 'Recueil de Méd. Vétérinaire,' 1874, p. 93.

<sup>2</sup> Op. cit., p. 94.



such an important part in the production of the malady. However, one day, when visiting the stables of an extensive farmer at Leinheim, he was informed that every year for five years one of the cattle had died from phthisis, "and what is very curious," said the farmer, "this always happens in the same stall." In proof of this, Grad was shown a young cow which, on examination, offered all the symptoms of tuberculosis: such as excessive emaciation, skin clinging closely to the bones, frequent and feeble cough, &c. Its value in this condition was from thirty to forty francs. According to the farmer, the animal had been ten months in the stall, and when first placed therein it was in flourishing health; it was the fifth cow that had fallen into the same state in this stall. Grad's attention could not but be strongly aroused by this report; nevertheless, he expressed his opinion that an hereditary tendency was doubtless the efficient cause in this succession of losses. Such, however, was not the opinion of the farmer, and it was at length agreed that Grad should choose a cow from another stable and put it in this stall, as an experiment. With this object a cow three years old and in calf was selected; it had every appearance of perfect health, had been bred on the farm, never been unwell, and never coughed, and none of its progenitors had been affected with tuberculosis. It was moved into the stall and remained quite well until after calving, when a short cough was the first symptom observed. This cough increased in frequency, and emaciation gradually set in, with the usual *cortége* of symptoms accompanying tuberculosis distinctly marked; so that in about twelve months the creature was only the shadow of its former self, and was sold for a trifle, like the others which had preceded it in the stall, to the low-class butchers. Grad could no longer resist the evidence in support of this cause, this being the sixth animal attacked with tuberculosis in the same stall; and he inferred that, in all probability, the disease had been transmitted by the ingestion of the matters expectorated by the cattle which had previously inhabited the same place. On his recommendation, all the woodwork of the stall was removed, the manger and rack were thoroughly disinfected, and the spot left unoccupied for a certain time. Then the stall was rebuilt, and again occupied by several animals in succession; but tuberculosis made no more victims either in it, or the other parts of the stable. This observation was made fifteen years ago, and since then Grad's opinion as to the transmissibility of tuberculosis has, he asserts, been amply confirmed; and the examples he could adduce of the conveyance of the disease by inhabiting a *contaminated stall* are numerous, and as patent as that just related. For a number of years he has recommended that the places occupied by phthisical animals should be disinfected; and in no instance in which this was adopted had cases of transmission occurred, no matter how numerous they may

have been previously. Grad also mentions that a number of people in his district believe in the possibility of transmission of tuberculosis by cohabitation, and he has no doubt that experience or observation has taught them this. He states that the observations he could report as to the transmissibility of tuberculosis by direct cohabitation—animals standing beside each other—are not so frequent or conclusive, because of the arrangement of the stalls, which completely separate the cattle towards the head; so that the expectorated matters stand but little chance of being ingested by those placed in the stalls adjoining a phthisical cow. In those sheds in which the cattle are not so kept apart cases of transmission have been noted, but the observations were not sufficiently rigorous to establish a certain conclusion upon. He adds, however, "In my opinion, this transmission is possible as soon as tuberculosis has set in at an advanced stage, and the expectorated matters are ingested by other cattle."

These observations as to the extension of tuberculosis in the bovine species are, as will be seen, so numerous and apparently so authentic and exact, as to be somewhat startling; but the results of experiments undertaken by various authorities with the view of ascertaining whether the disease could be produced by inoculation with, or the ingestion of, tuberculous matter, are yet more so. These experiments, it may be said, had no reference to the transmissibility of the malady by cohabitation; indeed, the experimenters, generally, do not appear to have had any idea that such a mode of infection was at all possible in the lower animals, nor yet even in man.

The first of these is Villemin, professor at the Val-de-Grâce Hospital, Paris. Having arrived at the conclusion, by a process of induction, that phthisis in mankind was the result of a specific or virulent agent introduced into the organism, he resorted to experimentation to confirm his views. The results of his experiments were communicated to the Imperial Academy of Medicine in two memoirs, having for their title '*Cause et Nature de la Tuberculose*,' the first of which was read on December 5th, 1865, and the second on October 30th, 1866.<sup>1</sup> These contained a fact of the highest interest: namely, the certain transmissibility of this affection by inoculation. The animals experimented upon were chiefly rabbits, and the material tubercle from the human lung. The number of animals inoculated was large, and tuberculosis, more or less extensively developed, constantly resulted. Guinea-pigs were also employed, and the inoculations were as successful as with the rabbits. No matter at what part of the body these inoculations were made, the tubercularisation that followed was marked by the same serious characters

<sup>1</sup> These results were published in a collected form in a small pamphlet, '*Études sur la Tuberculose*.' Paris, 1868.



in all, many of the creatures dying after a variable period, and others, falling into a state of marasmus, were killed. The lesions noted in them were chiefly found in the lungs, which were more or less infiltrated with tubercles; but other organs and textures were not exempted, the spleen, liver, bronchial and mesenteric glands, &c., being also involved in many cases. Attempts were made to produce the malady in other animals than these rodents, small ruminants and carnivora, but the results were generally negative; though a lamb so inoculated was found, four months afterwards, to offer lesions of circumscribed pneumonia, with grey or transparent granulations scattered on the surface of the lungs, and cheesy deposits in the liver and some glands. Villemin was somewhat in doubt as to the nature of the granulations, and appears to have inclined to the belief that they may have been the result of inflammation caused by the presence of strongles in the bronchi. At any rate, inoculation with them did not produce tuberculosis in rabbits. Of four puppies inoculated with human tuberculous matter, one fell into a state of marasmus; but when killed at the end of three months the lungs, liver, spleen, and kidneys, were found to be very healthy. The fourth alone, which had been inoculated at three different times, and was killed at the termination of five months, offered, along with cretaceous granules, some transparent granulations, the nature of which was undecided. Three young cats were inoculated, and destroyed six weeks after inoculation; the first was healthy, the second only showed a few granules of a doubtful character on the surface of the lungs, and the third, which had been inoculated twice, had a small number of pulmonary granulations, which certainly appeared to be tuberculous in their character, as well as a caseous mass at the point where the inoculation was made.

After these experiments with human tubercle on the rabbit, guinea-pig, sheep, dog and cat, Villemin was desirous of discovering whether the tuberculous matter of animals, and especially that of the bovine species, could give rise to tuberculisations. He therefore inoculated a rabbit in the ordinary way with a portion of the cretaceous deposits found in a phthisical cow; the creature fell into an emaciated condition, and in six weeks was killed. Its lungs were found to be filled with a great number of tubercular masses, several of which had a cheesy aspect in the centre; the pleura, spleen, liver, kidneys, and mesenteric glands, also showed a certain number of these masses. From this experiment Villemin concluded that bovine phthisis is identical with that of man.

Finally, with a view to determine whether the artificial tuberculisations produced by inoculation possessed the power, like that which arises spontaneously, of being transmitted; or, in other words, whether its virulence was retained beyond the primary transmission, he inoculated two rabbits with the matter from one which had been

rendered tuberculous. One of these, killed in about six weeks, had very small transparent tubercles in the lungs, spleen, follicles of the small intestine, and point of the cæcum; while those in the mesenteric glands were already undergoing softening. The other rabbit died at the end of the second month, and its lungs were completely infiltrated with tubercles in the form of confluent masses; while the pleura, liver, spleen, and kidneys, were studded with them. This second generation matter, inoculated in another rabbit, retained all its morbid activity, causing in six weeks intense tuberculisation of the lungs, with considerable engorgement of the bronchial and prepectoral glands.

The result of Villemin's researches was submitted to a committee of physicians and veterinary surgeons—members of the Imperial Academy of Medicine, who, in their report, while admitting certain of his conclusions to be exact, and of great, indeed of most urgent importance, nevertheless objected to others, chiefly from the different results their own experimental researches had in the meantime yielded. Indeed, a certain degree of hesitation, and even of unwillingness, appears to have been evinced then and for some time subsequently to accept what Villemin had no doubts whatever was substantially correct; and when his experiments were successfully repeated by others, it was sought to ascribe their success to anything rather than infection.

Numerous experimenters quickly entered the field to test the value of Villemin's opinions, and amongst these we find the names of Colin, Vulpian, Clark, Lebert, Cohnheim, Frankel, Empix, Wilson Fox, Sanderson, Waldenburg, Papillon, Nicol, Laveran, and others. The rabbit and guinea-pig were the animals chiefly operated upon; and though the experiments afforded generally the same results, yet, as has just been said, they were seldom ascribed to the cause insisted upon by Villemin. It was acknowledged that there was no difficulty in developing in rabbits and guinea-pigs a disease analogous to tuberculosis by inoculating them with tubercular products (grey granulations, caseous matter, and the sputa of phthisical subjects), but that the same effect might be induced by inoculation with non-tubercular pathological matters or the application of irritating setons; that, in fact, the introduction of matters which, acting chiefly as *mechanical irritants*, might give rise to appearances in every respect analogous to tuberculosis, and differing in no respect from that morbid condition. The results of these experiments, in fine, seemed to lead, according to the majority of the experimenters, to a directly opposite view to that entertained by Villemin with regard to the specific nature and virulence of this artificial tuberculosis; and it was sought to explain these results by stating that deposits of caseous matter were first created at the part where inoculation had been performed or the seton had been introduced, then in



the neighbouring glands, and that it was the caseous matter thus formed which gave rise to deposition of tubercle in all the organs.

This explanation was certainly conformable in every respect to the theory of Niemeyer, with respect to the development of tuberculosis in man. According to this authority, nothing so much predisposes to tuberculosis as the presence of a caseous deposit in any part of the body (lungs, glands, articulations, bones, &c.); and if the lungs are so frequently tuberculous this is because in them, oftener than anywhere else, this caseous matter is formed. Bühl even went further than Niemeyer, for he pretended that tuberculosis was always due to infection of the blood by a cheesy deposit; but, as was pointed out by the latter, there are cases of acute tuberculosis in man in which, on a necroscopical examination, no trace of such a deposit can be discovered. Bühl's law was, however, generally accepted as exact, in so far as the artificial production of tuberculosis in animals is concerned.

The manner in which a caseous centre might become the cause of tuberculosis was thus explained by several experimenters:

Bühl admitted that there was blood-poisoning by the cheesy matter, and he included tuberculosis among the infectious maladies; Niemeyer and others believed in a gradual generalisation of tubercle through the medium of the lymphatics; while Lebert and Behier thought experimental tuberculosis was due to capillary embolism. Papillon, Nicol, and Laveran were of opinion that the caseous matter might be conveyed by the lymphatics, as in their experiments they always found the glands in the vicinity of the inoculated part early undergoing the caseous transformation; though they were also inclined to think that certain elements of this matter might enter the circulation, and thus produce tubercles in the parenchyma of organs. These experimenters, it is worthy of note, never succeeded in tuberculising the serous membranes. Experiments conducted by Demet, Paraskeva, and Zallonis, in Syra, Greece, were not only successful in transmitting the disease to rabbits by inoculation with the sputa and blood from a man affected with phthisis, but they ventured upon the unprecedented experiment of inoculating a human patient whose history gave no indication of tuberculous taint, and whose lungs were perfectly healthy so far as a physical examination could discover, but who was suffering from gangrene of the big toe of the left foot, due to obliteration of the femoral artery. Amputation of the limb was proposed by the surgeon, but would not be submitted to; and as a fatal termination was inevitable, a quantity of sputa from a man who had abscesses in his lungs was inoculated in the upper part of the left thigh. About three weeks afterwards, auscultation revealed very slightly increased respiratory murmur at the summit of the right lung, and somewhat prolonged expiration in the subclavicular

region. In thirty-eight days after inoculation the man died from gangrene, and on examination of the body it was found that the upper lobe of the right lung had seventeen tubercles in the first stage of development, two of which were the size of a lentil, the others being as large as a grain of mustard; they were of a grey colour and very hard. Two similar tubercles existed at the apex of the left lung. At the centre of the convex surface of the liver were two tubercles, one as large as a shelled pea, the other smaller, both being very hard and of a yellow colour. From this unusual experiment, it is evident that tubercle is inoculable in man himself, for it is scarcely possible that this patient, who was fifty-five years of age, could have had his organs infiltrated with as many as twenty tubercles in the first stage of development only; had they been naturally developed, they must have multiplied and passed through their regular evolution, especially in an individual predisposed to phthisis. Their limited number, stage, and size, offered a direct relation to the brief interval separating inoculation from the decease of the patient.

To the objections brought against his views, Villemin replied in an address delivered before the Imperial Academy of Medicine in August, 1868. In this he enters minutely into the special features of this induced tuberculosis in animals, and maintains that the tuberculous matter comports itself in this respect as a virulent element. The results of his experiments are given in detail in proof of this, and he strives to make it understood that the tuberculous matter deposited beneath the skin produces a veritable contamination. It excites the formation of tubercles in the subcutaneous connective tissue, the walls of the lymphatic vessels and the glands, and, indeed, in the entire economy. But as the development of tubercles in the internal organs only appeared after the local phenomena, and not until after a certain number of days subsequent to the insertion of the matter, this period he compared to that of the incubation of virulent maladies. The question is also asked if this tuberculosis is not like syphilis, the local tubercle being, like the chancre, a primary phenomenon, and the source of ulterior accidents. To the facts and their interpretation offered by Villemin, those of an opposite tendency, which have been put forward, are reviewed by him. For instance, the development of a local lesion has been denied, and it has been pretended that the tuberculous mass found at the point of inoculation was the remains of the inoculated matter; that this matter, slowly passing through the lymphatics, did not reach the lungs until several weeks had elapsed; that it was an error to designate this interval that of incubation; and that the tubercles developed in the various organs were mainly constituted by the inoculated matter transported mechanically and in substance, the intensity of tuberculisation being proportionate to the quantity of matter deposited under the skin. Villemin combats these notions,



and with much force. "When we deposit in a wound," he says, "a portion of tuberculous matter the size of a pin's head or even a hemp-seed, and at the end of two months we find it grown as large as a filbert, we evidently cannot regard it as the remains of that which has been deposited. And when we make a hypodermic injection of some drops of sputa diluted in water or defibrinated blood, and discover in the connective tissue where the injection has taken place tuberculous masses and granulations, we cannot admit that these are the *reliquats* of the inoculated substances. And must we believe that it takes this blood-serum and sputa twenty or thirty days to traverse the lymphatics? No; these liquids are absorbed almost immediately, and the tuberculous matter itself does not long remain in the wound, as on the following day it cannot be seen. The tubercles found at a later period in the connective tissue, the walls of the lymphatic vessels, and the glands, are the products of a new formation, and do not represent the inoculated matter, which they, in certain cases, exceed a hundred times in volume. And this applies with still stronger force to those of the internal organs, the abundance of the tubercles being such that certain viscera are scarcely anything more than a tuberculous mass."

Stating that from his studies he was led to consider tuberculosis a specific malady, and desirous of verifying the hypothesis with regard to its inoculability, Villemin then informs us that he sought to realise, in principle, the conditions of a real inoculation: to wit, *a very small wound and an inconsiderable quantity of inoculable matter*. These conditions were never departed from, and all his inoculations were made with a portion of matter varying between the size of the head of a pin and that of a hemp-seed at most. And notwithstanding this constant uniformity in the procedure and the volume of the matter, tuberculisations extremely variable in their intensity and generalisation have been obtained; every degree had been observed, from a few scattered granulations to those startling generalisations in which nearly every organ was crammed with the proper pathological product of tuberculosis: *a manifest proof that the intensity of tuberculisation is completely independent of the quantity of matter inoculated*. The question of quantity only appeared to be invoked in blood inoculations, which, to succeed, demand a certain volume of that fluid.

With regard to the local tuberculisation, this is very often extremely insignificant; while the alteration in the lymphatic vessels and glands is far from being constant. In animals whose viscera were abundantly tuberculosed, the lymphatics have frequently been found unaltered, and the tuberculisation at the inoculated part quite rudimentary. Therefore, adds Villemin, *the number and extent of the internal lesions have no relation to the local lesions at the seat of puncture*. At the same time he remarks that the tuberculisation of

the glands in the neighbourhood of the punctures need not be wondered at; for in animals, as well as in mankind, the disease has a manifest affinity for the lymphatic glandular system, and it has been often observed that the glands which could not be affected by the direct passage of the inoculated matter, such as the mesenteric glands, are perfectly tuberculous. The same happens with syphilitic and glander inoculations.

It has been attempted to explain the transmission of tubercle by a process of grafting; "but how can such a process explain the myriads of tubercular granules which stud the parenchymatous and serous organs?" asks Villemin. "Grafted tissues continue to live and develop at the places where they are deposited, but they are not reproduced elsewhere in the organism. The insertion of periosteum beneath the skin has never led to the production of bone in the lungs, kidneys, or peritoneum. This theory, at the most, could only explain the development of tubercle at the seat of puncture; and even then the inoculated tuberculous matter, to do this, must be endowed with a very active vitality. But the softened matter in the centre of a tubercle does not even contain any distinct elements; we inoculate only with a *detritus*. How does it happen that the tubercle obtained from a body dead for thirty-six hours is susceptible of reviving and multiplying with that activity which prevails in the tuberculous eruption? How can grafting explain the successful inoculation with sputa—*sputa which has been dried for twenty days*, as has occurred in our experiments? Does not all this prove that the inoculated matter acts by virtue of a principle independent of the histological elements entering into its composition?" And the histological appearances of inoculated and non-inoculated tubercle are absolutely the same; while an irrefutable proof of their identity lies in the fact, that inoculation with the tubercles produced experimentally reproduces tuberculosis as readily as if human tubercle had been employed.

The *quasi*-tubercles developed in man, and more particularly in animals, by different parasites, the lesions of which bear a close resemblance to those of tuberculosis, are then discussed; as well as the results of injection of dust, mercury, tallow, and irritant substances of various kinds, even pus, into the bronchia and veins; and the great differences between them and tuberculosis are explicitly shown. To confound the former with the latter would be as great an error as to identify the pustules produced by tartarised antimony with those of variola, or the rubefaction induced on the skin by a brush with the eruption of scarlatina.

It will be seen that Villemin's arguments are very telling and convincing with regard to the production of tuberculosis by inoculation. But they found a more extended application, and still stronger corroborative proof, in the experiments instituted by



Chauveau, of the Lyons Veterinary School. That authority had been, for a number of years, carefully studying the intimate nature of the various contagia and their relations to virulent maladies, with a success which attracted much attention and afforded startling results; not the least of which was the discovery that certain of the contagia can infect as readily through the digestive organs as by any other channel. This had certainly been previously done, with regard to anthrax, by Renault, Davaine and others; while Renault had produced glanders in this way, and Roche-Lubin and Belliol, by causing sheep to swallow the crusts and matter from the pustules of those affected with variola, induced the disease in them; but Chauveau experimentally demonstrated on what conditions infection in this way depended—that they resided exclusively in the *virulent richness* of the matters submitted to the digestive action. With vaccinia, for instance, he affirms that it may be produced as certainly through the digestive tube as by the injection of the virus into the blood-vessels. In short, the result of his researches goes a long way to explain the oftentimes obscure transmission of zymotic diseases. Generalised syphilis, for instance, occurring without any initial accident, may be scientifically explained by the reception of the virus through the digestive organs. His object in experimenting appears to have been to arrive at some conclusion with regard to this absorbent aptitude of the digestive organs, in order to decide as to the virulence of such and such a disease, the contagious properties of which might be undecided. Indeed, he appears to have considered the ingestion of virus into the digestive canal as an admirable criterion of this property—an infallible test when it is possible to give the suspectedly virulent matters in sufficient quantity to assure the penetration of the contagiferous elements. This criterion Chauveau sought to apply to the controverted question as to the virulence of tuberculosis; as the infection thus produced evades all the objections offered to the results of direct inoculation, as well as those of pulmonary infection in those diseases in which the lungs are chiefly affected.

Impressed with the idea that, if tuberculosis is a virulent malady, it should almost infallibly be communicated through the digestive canal, by reason of the exceptional facilities for administering in large quantities the matters containing the virus, he framed his experiments with a view to test this notion thoroughly. In the first place, he decided that his experiments should be made upon a species of animal in which tuberculosis was a natural and very common disease, endowed with all the seriousness and the characters it presents in the human species. With this object he selected the bovine species, as, in his opinion, this was the only way in which he could exactly appreciate the value of the results obtained, by comparing them with the characteristics of the natural or spontaneous malady. This choice, however, put him to the necessity of adopting special

precautions to avoid experimenting upon animals already infected; and he was therefore compelled to resort to young creatures born and bred beyond all those influences or conditions which are supposed to favour the natural development of phthisis. As a further precaution, it was decided to place beside these one or more animals of the same species, as a means of check or control, and to afford the assurance that those experimented upon had not been exposed to any other cause capable of developing the disease after the ingestion of the reputedly virulent matters.

In 1868 four calves, aged from six to twelve months, were purchased from a locality where phthisis was unknown, and after purchase they were examined with the greatest care; but they were found to be in excellent health and remarkable condition for their age. On the day after their arrival, three received about thirty grammes of tuberculous matter obtained from an old phthisical cow in one of the Lyons slaughter-houses. The caseous, puriform, and cretaceous mass from the lungs was pounded in a mortar and dissolved in water, and then administered from a bottle in small quantities at a time. Attempts had been made to induce the calves to drink the virus in their water, but they refused to do so.

The calves, for facility of description, were designated as Nos. 1, 2, and 3; that which was kept for comparison being No. 4. On the fourth day No. 2 had a profuse and very fetid diarrhoea, but it was well on the evening of the next day. The administration of the tuberculous matter took place on September 19th, and at the commencement of October the following notes were made with regard to the condition of the calves:—No. 1 (twelve months old), has lost condition; it rests more; the respirations are quickened, and while the animal is lying number thirty-eight or forty per minute; the appetite is unimpaired, though rumination is always slow at the commencement. No. 2 (six months old), and which had diarrhoea, appears to be in excellent health. No. 3 (six months' old) is the same. On October 5th and 7th, Nos. 1 and 3 received a certain quantity of tuberculous matter from lungs not so much diseased as the others, but in which it was possible to discover recent tubercular infiltrations. No. 2 did not receive any; for if this administration would produce tuberculosis, Nos. 1 and 3 would furnish the proof. It was hoped that No. 3 would escape the effects of the virus, as it was valuable in a pecuniary sense, and its sale would have diminished the expense of the costly experiment.

Further details of the symptoms are omitted, but the *dénouement* surprised Chauveau by the rapidity with which it was developed. Calf No. 1 exhibited the first evident symptoms of tuberculosis. From October 9th there could no longer be any doubt as to the success of the administration; as, although it still preserved its appetite, it be-



came emaciated in an extraordinary manner, while the coat became rough and staring, and the animal had fits of coughing morning and evening, particularly after drinking; and No. 2 soon began to exhibit all the symptoms of tubercular infection, its coat assumed the same aspect as in the other, and it lost its excellent condition, notwithstanding the undiminished appetite, and it coughed at times. The most remarkable symptom it offered, however, was in the appearance of the lymphatic glands of the neck near the larynx. There was also at times an intermittent "roaring," which the slightest pressure on the larynx produced at once. No. 3 resisted longest, as it offered no appreciable derangement of health until October 25th. From that date, however, the phenomena of tuberculous infection ensued with the greatest rapidity. In eight days the creature could scarcely be recognised; though it is true that it had slight diarrhoea, and did not have the same appetite as the others. The cough was nearly incessant; the peri-laryngeal glands became involved, and one of them, the left submaxillary gland, was as large as a goose's egg. During the whole of this time No. 4 remained in perfect health.

On November 10th, fifty-two days from the commencement of the experiment, it was considered time to close it; and at this period it was sufficient to enter the stable and cast a glance at the four animals, to appreciate the remarkable results obtained. The miserable aspect of the three infected creatures contrasted in the most extraordinary manner with the thriving condition of the non-infected one, which at the commencement was the puniest.

It was decided that Nos. 2 and 3 should be at once killed, No. 1 being kept in order to study the alterations at a more advanced period of infection. This calf was also apparently a little better, scarcely coughed, and, had it not been for the progressive emaciation, a real improvement might have been said to have taken place.

No. 3 was first bled to death. The autopsy, made immediately, revealed the most perfect lesions of generalised tuberculosis, the mesentery and intestines being extremely involved. The mesenteric glands were so infiltrated that several had acquired the volume of a man's fist. Their entire mass weighed 1650 grammes. It was a magnificent example of "carreau," or mesenteric phthisis. With regard to the intestinal eruption, this was manifested from the commencement of the duodenum to near the termination of the colon, but its confluence was particularly remarkable in the ileum. The tubercles were nearly all in a state of ulceration, and they were almost as numerous on the other parts as on Peyer's glands. The true stomach had also some tubercles; but none were observed in the other compartments of the viscus, though three-fourths of the glands lying beside the vessels of these were tuberculous. Neither liver, spleen, nor kidneys, were affected. The lesions in the thorax,

though less marked than those in the abdomen, were yet fully as remarkable. All the bronchial glands, as well as those of the mediastinum, were diseased, and had attained a considerable volume. The lungs were studded with crude tubercles, in number about forty, whose volume varied between that of a pea and a filbert. There were also a certain number of greyish, semi-transparent, but isolated granulations projecting beyond the surface of the pleura. Nothing unusual was observed in the bronchia or the greater part of the trachea; but at the commencement of the latter, as well as in the subglottidean portion of the larynx, on the posterior part of the windpipe, there were granular patches speckled with small ulcers, some of which were bleeding. Two similar patches existed on the inner face of the arytenoid cartilages. Between the pharynx and the vertebral column were two glandular masses, whose development was such that they must have certainly impeded deglutition. All the other cervical glands were infiltrated with yellow tuberculous matter; the submaxillary glands were likewise involved, those on the left side being greatly enlarged. The cranial organs were unaffected.

These were the results of this most interesting and startling autopsy; and to fully appreciate their value regard must be had to the age of the animal, for we must not forget the predominance of glandular over the other forms of tuberculosis in what we may term "infancy."

The autopsy of calf No. 2 was not less interesting; though its results were so like those of No. 3, that Chauveau only enumerates some peculiarities. The principal of these was the complete absence of laryngeal phthisis, though the cervical glands were as seriously affected as in the other animal; and though the granulations and masses of tubercle in the lungs were as well defined, yet they were not so numerous. The mesenteric tuberculisation was quite as advanced, and the tumours as voluminous; but the intestinal eruption was, on the contrary, quite discrete, and was only remarkable in the ileum. There were a few grey, semi-transparent granulations in the spleen.

In the presence of these important facts, asks Chauveau, is it possible any longer to doubt the virulence of tuberculosis? and he replies in the negative. "It now appears proved that the identity of tuberculosis with the other virulent diseases is so complete and so absolute, that we must either recognise its virulency, or deny the existence of virulence altogether. There is no middle place in this dilemma."

To prove that there was no analogy between this induced disease and purulent affection, and to dispel any doubts which might exist in this respect, Chauveau asserts that he had a multitude of facts to prove that ordinary pus may be introduced into the digestive canal, even in considerable quantity, with impunity.



Chauveau is certainly the first to point out the important consequences resulting from these experiments, with regard to the manner in which tuberculosis may be propagated, and their indications in respect to private and public hygiene. If, he says, tuberculosis is acquired through the digestive organs, it is evident that its natural and spontaneous contagion cannot be exclusively attributed to infection of the atmosphere by the air expired from the lungs of phthisical animals. Creatures confined in the same stable or pasture, and drinking from the same ponds or troughs, are constantly liable to swallow the mucosities that are discharged from the nostrils of their comrades; and if these secretions are derived from phthisical animals, they may become the cause of tuberculous infection. This is equally true for the human species, and it would be superfluous to demonstrate how the intimacy which exists between the members of a family, especially between husband and wife, necessarily exposes them to all the chances of tuberculous infection by the digestive organs. Indeed, so convinced was Chauveau of this, that he proposes to show that this mode of infection may be incomparably more frequent than contagion by the respiratory passages. Meanwhile, it is sufficient that infection by the digestive organs is demonstrated to be possible, in order to impose, in the prophylaxy of tuberculosis, very simple hygienic precautions, which may avert the chances of accidental transmission of the tubercle virus by this channel.

But what is of still more importance, perhaps, is the relation of bovine tuberculosis to public hygiene, and Chauveau appears to have been the first to indicate this source of danger. He states that the flesh of phthisical animals enters somewhat largely into the public alimentation, no legal enactments explicitly proscribing its sale; and cattle in every stage of tuberculosis are killed in the slaughter-houses, those most diseased finding their way to the butchers' stalls in poor localities. He, therefore, is quite justified in inquiring what part the consumption of such food may play in the propagation of phthisis in the human species, especially when it is remembered that bovine tuberculosis is identical with that of man. And Chauveau's more recent experiments have proved that cattle can be infected by the ingestion of human tubercle. But this authority did not regard the flesh as being virulent in itself, except when it contained tuberculous matter in the shape of infiltrated lymphatic glands, &c., though the blood retained in the flesh might also be a vehicle for the tubercular elements. He also notes that the tuberculised viscera are not rejected as food when they are not much affected. Cooking, it is true, may completely destroy the virulent property of the flesh; and this is most fortunate, for otherwise its utilisation might entail the most lamentable and wide-spread consequences. Safety can only be attained, however, by thorough

cooking, so as to completely destroy the virulent activity of such flesh.

Chauveau, finally, insists on the necessity for studying this grave question in sanitary policy, and concludes his important communication with the following *résumé* of his experiments:—1. They prove that bovine animals contract tuberculosis by digestive ingestion, as they may take anthrax and vaccinia, as sheep take variola, as solipeds take glanders, as man takes smallpox, &c. 2. They place beyond doubt the fact as to the virulence and contagious property of tuberculosis, and show that the labours of Villemin have not been <sup>fully</sup> recognized as they deserved. 3. That the digestive canal constitutes, in the bovine species, as in man, a channel of contagion readily disposed for the propagation of tuberculosis, and which may be more frequently the mode of access than through the pulmonary organs.

Subsequent experiments, by the same excellent authority, have still further supported the conclusions he had arrived at in his earlier researches, and these must be held as convincing to any but the most sceptical. Now and again some experimenters, who have not been so successful as Villemin and Chauveau, have controverted the results he has established, and others have denied that these results have any value in the direction indicated by him. But, fortunately, in one respect, Chauveau soon had numerous proofs of the justness of his conclusions. Harms and Gunther, of the Hanover Veterinary School, produced tuberculosis in rabbits by feeding them with the flesh and lungs of a tuberculous pig and a phthisical cow. They did not find the young of these rabbits infected, though they had been suckled and kept alive for ten weeks.

Leisering, of the Dresden Veterinary School, fed a sheep for three days on the tuberculous lymphatic glands of a cow. After the fifteenth day, the thermometer indicated an increase in the temperature of the animal of  $1^{\circ}$  to  $1.5^{\circ}$ . On the sixth week there was cough and emaciation, and towards the tenth week the respiration was hurried; while auscultation and percussion revealed infiltration of the right lung, which had become impermeable to the air. The sheep was killed on the eighty-fifth day, and on an examination of the carcase being made, the intestinal mucous membrane was found ulcerated and studded in different places with small tuberculous tumours; the mesenteric glands were tuberculous; the liver and lungs were full of tubercles, and in the latter the largest masses were already calcified. The tumefied bronchial glands likewise exhibited traces of cretification. Another sheep, which had only received twenty grammes of tuberculous matter, also became affected, as did a number of rabbits fed on the same material.

Gerlach, of the Berlin Veterinary School, thus describes the results of his experiments:—1. The tuberculosis of cattle is very infectious. 2. The tubercles covering the serous membranes, as well as those in



the other organs, are as infective, and produce the same tubercles, as the tuberculous matter of the lungs. The identity of pulmonary phthisis of cattle and general tuberculosis cannot be doubted. 3. Infection can be produced after inoculation, as well as after ingestion of the tuberculous matter. 4. The flesh of animals affected with tuberculosis possesses, in certain conditions, the power of infecting, though to a less degree than the tuberculous matter. 5. The temperature of boiling water destroys the infective principle; though boiled tubercles, nevertheless, often preserve a certain degree of virulence. It is in this as in trichinosis. Although the temperature of boiling water, or even a lower temperature, destroys the parasites, yet there may be found in the centre of the boiled flesh living trichinæ: muscle being, in general, a bad conductor of heat, and the high temperature only reaching its interior after some time has elapsed. In some experiments it will be found that tubercles an inch in diameter will, after half an hour's boiling, still possess infective properties; though these are, of course, diminished. Gerlach's experiments have been, and are now being, carried on with all kinds of animals, and particularly with cooked and uncooked flesh and uncooked milk. He asserts that the infective properties of this fluid can no longer be doubted or denied.

Zürn, of the Jena Veterinary School, has fed pigs, first with the milk, and then with the flesh of a phthisical cow, and produced various degrees of tuberculosis in them.

Böllinger, of the Zurich Veterinary School, has made nineteen experiments, the results of which led him to the following conclusions:

1. Tuberculous matter obtained from man, and inoculated in the dog, produces a typical miliary tuberculosis of the pleura, lungs, liver, and spleen. Inoculations on carnivorous animals in general are negative, or only produce an insignificant local reaction.

2. The inoculation and ingestion of tuberculous matter from the ox, produces, in herbivorous animals (goats), tuberculous infection in two forms—miliary tuberculation of the peritoneum, and caseous deposits in the intestinal mucous membrane, as well as in the mesenteric glands.

3. The contents of the bronchia of the tuberculous lungs of an ox produce the same effects as the caseous matter of the lungs, when inoculated or ingested.

4. The ingestion of fresh tuberculous matter from the ox has no effect on carnivorous animals; with herbivorous creatures, on the contrary, it produces intense tuberculous infection, characterised by caseous lesions of the intestinal mucous membrane and mesenteric glands, hypertrophy of Peyer's patches, and the eruption of miliary tubercles in the peritoneum, liver, and lungs.

5. The tubercle virus is active in small doses; twenty to twenty-five grammes of tuberculous matter from the lungs may kill such creatures as the goat, in two months.

6. The ingestion of pus alone from the caseous lesions does not produce tuberculosis in the goat.

7. Certain forms of tuberculosis induced by the ingestion of tuberculous matter present, in an anatomical and pathological point of view, a great analogy to human scrofula, and, like it, are manifested by caseous degeneration of the cervical and mesenteric glands.

8. There is no incompatibility between anthrax and tubercular infection.

The experiments of Villemin, Chauveau, Klebs, Gerlach, Bagge, Semner, Gunther and Harms, Zürn, Biffi and Vergad, and some personal experiments previously made, are quoted by Böllinger, who arranges them under four heads or sections, as follows:

1. Ingestion of tuberculous matter obtained from man. In two pigs and two rabbits, a negative result. In one pig, enlargement of Peyer's patches and mesenteric glands, and caseous degeneration of portions of these.

2. Ingestion of tuberculous matter from the ox: fresh glands, caseous matter, the contents of the bronchia. Animals experimented with: five sheep, two goats, four pigs, eight dogs, a large number of cats, twenty rabbits, one porpoise, eight pigeons. A negative result with the dogs and cats; nearly always a positive result with the pigs, sheep, and goats. Most frequently, with these animals, there was caseous degeneration of the intestinal mucous membrane, mesenteric glands, sometimes the cervical glands, and the lungs. In three sheep there was veritable "*tabes mesenterica*." With the rabbits, the ingestion of raw tuberculous matter produced results sometimes positive, sometimes negative. In one instance, the ingestion of boiled tuberculous lymphatic glands developed general tuberculosis; but, on the other hand, boiled tubercles had no injurious effect on five rabbits. And a pig fed with the same material only exhibited, after death, tumefaction of the mesenteric glands. Cooked or uncooked tubercles given to other rabbits infected them; and the ingestion of tubercles from an ox infected a porpoise, but had no influence upon two pigeons. The ingestion of caseous pus alone had no effect on a sheep.

3. Ingestion of the flesh from phthisical oxen, or those artificially infected. Positive result in three pigs; general tuberculosis or alterations in the lymphatic glands. In one pig leucæmia, scrofula, and tuberculosis. Result always negative with rabbits when fed with raw or cooked flesh.

4. Ingestion of the milk of a tuberculous cow. Three pigs, three calves, one sheep, two goats, two cats, and fourteen rabbits. In the



three pigs miliary tuberculosis, and lesions analogous to those of scrofula; in the two cats, a negative result. A positive result in two rabbits; a negative result in fourteen rabbits fed with the boiled milk.

Böllinger thinks it possible, or rather probable, that intestinal tuberculosis, consecutive to pulmonary phthisis, may be produced by the sputa being swallowed and passing into the intestine. He also thinks it proved that scrofula and tuberculosis are only two forms of the same disease at different periods of development.

Klebs has been successful in producing tuberculosis by giving animals milk from those which were diseased, and his experiments, therefore, have an extremely important bearing. In addition to rabbits and guinea-pigs—creatures which appear to be very susceptible to the artificial production of the malady—he accidentally induced the disease in a dog by feeding it with the milk of a cow in the last stage of phthisis. The results of his experiments led him to the conclusion that the use of this milk always produces tuberculosis, which commences as an intestinal catarrh, and then assumes the form of tubercles in the mesenteric glands; it afterwards affects the liver and spleen, and subsequently the thoracic organs. He asserts that the tubercle virus is present in the milk of phthisical cows, whether they are slightly or gravely affected; and that it chiefly exists in the serous portion, as when the milk has been so filtered as to deprive it of its solid particles, the fluid portion appeared to be as active as when the malady had reached an advanced stage in the animal from which it had been procured.

He admits that it may produce no injurious effects on vigorous subjects, and he has even observed fully developed tubercles to be absorbed and disappear after a time. He thinks it probable that the virus of tuberculosis may exist in varying proportion in the milk of phthisical cows, according to the extent of the disease in them; and he is further of opinion that the malady may be developed in children born without any tendency to it, through the medium of the milk of the mother or nurse.

Since Kleb's researches were published, Viseur, an able veterinary surgeon at Arras, France, has been successful in producing tuberculosis in cats, by feeding them with tuberculous matter. One of these animals was found to have all the lymphatic glands enormously hypertrophied; the mesenteric glands greatly increased in size; and the lungs studded with white hard tubercles—some of them as large as a grain of barley. These cats ate the tuberculous matter voluntarily.

Saint-Cyr, of the Lyons Veterinary School, has likewise proved the transmissibility of the disease by the stomach.

The vital tenacity of the tubercle virus appears to be somewhat remarkable. Inoculation experiments have demonstrated that it

preserves its potency through three or four removes or generations ; and successful inoculations have been made with tuberculous matter from a patient who had been dead for thirty-six hours, and with sputa which had been in a dried condition for twenty days. If thoroughly boiled, its vitality is destroyed ; and Klebs found that the action of alcohol deprived it of its potency. Villemin is of opinion that the transmission of the malady in the human species takes place most frequently by means of the dried expectorated tuberculous matter being accidentally reduced to powder, and carried by the air into the lungs. Veterinary surgeons have for many years believed that forage soiled by the expectorations of the disease, and consumed by healthy animals, will communicate the malady.

We have yet a good deal to learn with regard to the pathology and communicability of this disease ; but there can be no hesitation in recognising the importance of the results already achieved. The commencement of phthisis is generally so insidious in the human species, that it is most difficult to arrive with any degree of certainty at the causes which directly induce or favour its development ; but, from the evidence before us, it is to be feared that at least one of its sources must be referred to the utilisation of the carcase, but more especially of the milk, of phthisical cattle as food. It is certain that tuberculosis is a common and a very destructive disease, among dairy cattle especially, and more particularly those in towns ; that the udder of these animals is one of the glands not unfrequently involved ; that infants and adults consume milk in somewhat large quantities—indeed, it is the principal article of diet of young children ; and that phthisis is a very prevalent and fatal malady in the human species, and chiefly among the dwellers in towns and cities.

From what has been already ascertained, there is every reason to view with grave suspicion the use of the flesh of phthisical cattle as food, especially if the disease is much advanced and the tissues generally involved. But with more reason the milk from cows affected with tuberculosis should be prohibited, more particularly for the use of infants, who mainly rely upon milk for their sustenance, and whose powers of absorption are very active. Even if such milk did not possess such dangerous infective properties, its deficiency in nitrogenous elements, fat, and sugar, and the increased proportion of earthy salts, would alone render it objectionable as an article of diet. It has long been known that it was liable to produce diarrhœa and debility in infants ; but though many children fed on such milk have died from general or localised tuberculosis, the part probably played by this fluid in its production has not been suspected.



## Chronicle of Medical Science.

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### REPORT ON PATHOLOGY AND THE PRACTICE OF MEDICINE.

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*Progressive Anæmia perniciosa.* By Prof. IMMERMANN, of Basle.—Biermer was the first to observe and group together a set of cases of progressive and fatal anæmia, which partook of certain features and altogether exhibited a pathological unity. In the paper before us Immermann reviews the collected (fifteen) cases of Biermer and those of Gusserow, and adds other two from his own practice. The striking features are those of intense general anæmia, pallor of all visible tissues, debility, vertigo, faintings, dyspnœa, palpitation, &c., yet without marked wasting of the fatty tissue or other signs of marasmus. What distinguished it from other forms of anæmia are—1, the want of a definite and sufficient cause; 2, the excessive degree of bloodlessness and the connection of the anæmic symptoms with certain changes in the circulatory apparatus; 3, the appearance of febrile action without an anatomical basis; and lastly, 4, the progressive course of the malady and its almost invariably fatal issue under any form of treatment hitherto tried.

It would appear to be a disease neither of frequent occurrence nor of general distribution. Biermer's experience would seem to show it to be particularly prevalent in the Canton Zurich. Almost all the cases happened among females, and the majority in those between twenty and forty years of age. Pregnancy and parturition, and in other instances unfit dwellings and deficient nourishment, appear to act as contributory causes. Likewise other debilitating causes, among which may be enumerated long-standing disease of the digestive organs, prolonged diarrhœa, and repeated losses of blood, may be noted; although, indeed, in some examples the accession of the malady would appear to have been spontaneous. At the same time none of these can be held to represent the actual or efficient cause of the peculiarly fatal anæmia under consideration. Looking to its prevalence in certain localities a special local cause may be presumed to exist. Again, in no other diseased state is the anæmia so great except only in death by loss of

blood, but there it is an acute condition, whilst the anæmia perniciosa is a chronic malady. Further, in all ordinary symptomatic anæmias there is wasting, but this feature is wanting in the specific variety in question. There is no loss of the cushions of fat occupying interspaces between organs, as happens in consumption and cancer, and all wasting diseases with hectic. Further, some cause over and above those usually reckoned among the causes of debility must be looked for to account for the state of the blood,—a something which acts directly on the blood-making or on the blood-destroying process, and yet not akin to that causing chlorosis, leukæmia, morbus Addisonii, or the hydræmia of acute and severe albuminuria. Contrasting anæmia perniciosa with an “essential” anæmia, such as chlorosis, differences are distinguishable in the less degree of immunity of the male sex to the former malady, in the closer relation of chlorosis to sexual evolution or puberty than of fatal anæmia, which on its part is rather associated with pregnancy and parturition; and in the failure of the treatment found curative of chlorosis, to remedy or stay the progress of the inveterate anæmia. It is also worthy of note that the striking coincidence, of late observed by Virchow, of a congenital narrowing of the aortic system with the appearance of chlorosis at puberty, has not been remarked in any of the cases of anæmia perniciosa yet recorded.

Agreement with those other morbid states above named, in which bloodlessness and weakness are chief features, is still less close than with chlorosis. There is no renal disease, and only exceptionally a trace of albumen in the urine; no actual multiplication of the white corpuscles or enlargement of the spleen or lymphatic glands; no lesion of the supra-renal capsules, and no discoloration of the skin. Only one other malady to which it has a seeming affinity can be named, namely, the African anæmia or geophagia, but this is ascribed by Griesinger to the presence of anchylostroma in the duodenum, a cause undiscoverable in the disease under consideration. Consequently it must be attributed to a special blood-disease.

Indeed, it is clear that this anæmia is due to an actual disease in the mass of blood throughout the body. This oligæmia is associated with oligocythæmia and hypinosis, and, towards the end of life, with hydræmia. Hence the pale, watery blood, capable of forming only an imperfect and small coagulum, and the appearance of œdema and subsequently of effusion into the serous cavities when life has been sufficiently prolonged. There is an excessive diminution of the red corpuscles with a *relative*, not an *actual*, increase of the colourless corpuscles. However, in a case examined by Ponfick, owing to a less degree of impoverishment of the colourless elements of the blood, a relative leukotysis resulted, and an affinity thereby manifested to true leukæmia.

The condition of the blood is itself explanatory of the general symptoms of the disease; of the blanched colour and excessive weakness; of the accelerated heart action, and weak, undulating impulse; of the loud systolic murmur, especially at the base and in the course of the great vessels, apart from any valvular disease; and of the bruit in the neck. In the production of the murmur some influence must, indeed,



be attributed in the later stages of the disease to the changes that proceed in the muscular tissue of the heart, and particularly in its papillary muscles. The state of the blood is also accountable for the hæmorrhagic tendency noticed in the form of epistaxis, of bleeding gums, and now and then of fatal cerebral hæmorrhage, of petechiæ of the skin, and of multiple, minute hæmorrhagic patches in the brain-substance. Another, and a characteristic consequence, is retinal hæmorrhage.

Among the coarser lesions observed in the circulatory apparatus is a partial fatty degeneration of the muscular tissue of the heart (especially seen in the papillary muscles), of the true coat of the arteries, and, in patches, of the capillaries. In such lesions an explanation is to be found of the hæmorrhagic diathesis present, beyond what the state of the blood itself furnishes.

Ponfick describes besides fatty degeneration of the heart and blood-vessels, a fatty metamorphosis of the epithelial cells of the kidneys, of the hepatic cells, and of the glandular follicles of the stomach, as happening in the advanced stages of oligocythæmia. These cell-changes were not noted by Biermer or Gusserow, but are confirmed by Immermann so far as relates to the renal epithelium and the liver-cells. The last-named observer has, however, failed, like Ponfick, to notice the omental hæmorrhages mentioned by Biermer; but he is disposed to believe that these differences indicate, not inaccuracies of observation, but are to be explained on the hypothesis of two forms of the malady—one characterised rather by degenerative changes, the other by hæmorrhagic phenomena.

The remaining symptom referred to deserves especial consideration. The febrile movements noticed in the course of the disease exhibit no typical character, and seldom, and then only temporarily, attain a high degree. Their duration is likewise variable, and the accessions separated by long intervals; yet they usually recur at periods of greater or less extent until life terminates. At present these movements of what Biermer calls "anæmic fever" cannot be reduced to rule; but the fever may be assumed to depend on the influence of the altered blood on the nerve-centres concerned in regulating the temperature; and it may be that the elevation of temperature is due to the augmented disintegration of the red corpuscles of the fibrin-forming material and of the albumen of the serum. This humoral origin of the fever is shown by Immermann to be countenanced by the phenomena both of chlorosis and of leukæmia.

The paper is accompanied by clinical details of three cases of this inveterate form of anæmia, with diagrams of the variations of temperature recorded, and the particulars of the examinations made after death.—*Deutsches Archiv für Klinische Medicin*, Band xiii, Heft 3, May, 1874.

*Cases of Tuberculosis of the Spinal Cord.* By Prof. CHVOSTEK.—Two cases are narrated briefly. The first occurred in a male, æt. 30, who died seven weeks after the malady exhibited itself. The first symptoms perceived were colicky pains with diarrhoea, followed by

darting pains in the left leg, which soon made their appearance also in the right leg. One month before death the left leg was completely paralysed, the sense of pain and of touch exalted, and Faradaic muscular reaction materially reduced. The right leg remained painful only at that time, but soon after became paralytic and hyperæsthetic, whilst the left lost its sensibility. Paralysis invaded the bladder with accompanying catarrh, and the right leg became totally paralysed in motion and sensation. The autopsy exhibited at the level of the fourth and fifth cervical nerves greyish-yellowish softening through the whole thickness of the cord, extending gradually both above and below. Three inches above the cauda equina was a hard tubercle, the size of a pea, surrounded by inflamed tissue through the thickness of the cord. The tubercular deposit in the lungs was in a far less advanced stage than that in the spinal cord. That the softened portion in the cervical portion was of quite recent production was indicated by the onset of asthmatic paroxysms shortly before death.

The second case was that of a public official, æt. 43, who was seized eight months before his death with paræsthesia, pain and progressive weakness of the left arm. Three months subsequently the same morbid consequences showed themselves in the left leg, and very soon afterwards in the right one. At the same time the rectum became paralysed and insensible. At length the right arm was affected, the muscular power of the bladder destroyed, deglutition rendered difficult, and the tongue tremulous. Shortly before death the paralysis of the legs and of the left arm was complete, whilst that of the right arm was less so; spasmodic jerkings were brought on in the legs so soon as they touched the ground, but not by passive movements, nor by extension of the contracted joints. The muscles were very much reduced; their muscular reaction to Faradism decreased in direct correspondence with the degree of paralysis, and the galvanic irritability of the nerves was greatly diminished. The muscles of the nape of the neck were strongly contracted, but rotation of the head was accomplished without pain. Reflex irritability not augmented. The sensation of pain obliterated in the extremities throughout, and that of touch also, except in the right arm, where the loss was less complete. Swallowing was much impeded; the urinary bladder paralysed, although the urine was normal. Much wasting.

On examination the meninges were found normal, but at the level of the fourth and fifth cervical nerves there was a tubercle of the size of a hazel-nut, of softer consistence at the centre, and surrounded by softened tissue, which extended forwards in the anterior columns to the second cervical nerve. The lungs, intestine, and kidneys were tubercular. Prof. Chvostek remarks on the comparative advanced age of the patient, and on the central softening, and comments on the jerkings of the limbs as bespeaking the pressure rather of tumour than of multiple sclerosis.—*Wiener Med. Presse*, 1873; and *Schmidt's Jahrbücher*, June, 1874).

*Galloping Phthisis and Acute Tuberculosis.*—M. Leon Colin argues in this paper in favour of the identity or unity of nature of these two



conditions, which by Virchow and others have been described as distinct morbid states. He writes from the very extended field of observation afforded him among numerous recruits, young soldiers, and others who have been inmates of the large hospital—Val de Grâce—of which he is physician ; and he also appeals to the special researches he made and published on the subject ten years ago. The distinction made between consumption and tubercle is one based too exclusively on pathological anatomy, and on the secondary differences between morbid products, and is opposed to the results of clinical observation. Indeed, it is no marvel that an affection capable of such varied localisation, and of modes of variation so diverse, should present forms that at first sight might seem to be fundamentally different. Thus the contrast seems complete between a case of consumption where the morbid process proceeds in its selected locality, leads to ulceration, and progressively reduces the area of respirable lung, and yet, withal, permits the prolongation of life for months, or even years, and one of the dissemination of tubercular granulations, which, although in themselves appearing likely to impede in but a small degree the function of any organ, yet in a few days are so multiplied or developed that they induce a morbid process comparable to that of an acute virulent malady. On the other hand, a case of phthisis may assume the acute or galloping character, with rapid wasting and hectic ; nevertheless, it still presents its local intrathoracic character, and is revealed by the ordinary physical signs just as if it had remained chronic. To this condition, again, acute tuberculation presents a contrast by its rapid diffusion in several organs together, and by its commonly not acquiring those proportions which render it discoverable by physical exploration. In this latter affection, likewise, the general symptoms are by far the most noteworthy, and the observer has rather to determine, not whether he has to deal with phthisis or tuberculation, but whether with acute tuberculation or typhoid fever. Moreover, it has been M. Colin's lot to notice that most of his cases of acute tuberculosis have occurred among young soldiers recently enlisted, and predisposed to acute specific maladies of typhoid or eruptive nature, and that the cases have been admitted into hospital in groups, as if their affection in some way depended on a transitory medical constitution then prevalent, or on an influence of an epidemic character.

To substantiate his opinion of the actual identity of the two morbid states he appeals to symptoms and to lesions. He points out that, between acute, yet localised phthisis and primitive, generalised tuberculosis, are numerous intermediate lesions in which a distinction is much less recognisable, either from simultaneous evolution of the two affections, or because the acute tuberculosis runs its course within a limited region, or, still more, within the anatomical domain of galloping consumption, where, in a symptomatic point of view, it does not overreach the respiratory apparatus. It is, indeed, the recognition of the modifications of features that acute tuberculation manifests clinically when it ceases to operate generally, and so comes to limit itself within a restricted area, which informs us how widely it may differ from itself, and how closely it may approach in character to galloping

phthisis. In fact, in many instances, its character as a general malady becomes subordinated, by reason of especial localisation, to the special symptoms due to the suffering organ. Further, the rapidity of the course of tuberculosis is generated by the character and functions of the organ at first invaded. For instance, its outbreak is more violent when the lungs or the serous membranes are primarily attacked, than when the mesenteric glands are invaded. In the latter case the febrile explosion is less intense, and habitually preceded by a period of simple malaise. On the same principle, tubercular meningitis exhibits a closer affinity to simple meningitis than to general tuberculosis, and likewise tubercular nephritis approaches nearer to acute nephritis than does that which indicates the onset of albuminuria. These examples suffice to show how considerably the evolution of acute tuberculosis must be modified when the respiratory apparatus is more especially its seat, or the centre of its "phenomenisation;" and, on the other hand, experience furnishes a multitude of cases in which the diagnosis between galloping consumption and acute tuberculosis, between the caseous and the granular lesion, is a difficulty or even an impossibility.

In the second place there are anatomical proofs of the identity of the two conditions. Grey granulation, yellow miliary tubercle, and tubercular infiltration constitute but varieties, or rather mere phases of evolution of a product identical in origin. To illustrate his opinion, M. Colin has divided his cases of acute tuberculosis into two groups—  
(1) A primitive form wherein a previously sound individual is attacked.  
(2) A secondary form where the disease manifests itself in a previously tubercular subject. In the former granulations are the predominant feature, in the latter the circumstance of disease are more complicated, for along with granulations, and anterior thereto, are deposits of greater or less bulk—infiltrations and caverns—the habitual lesions of ordinary (classic) phthisis.

Now, in almost all cases of primitive acute tuberculosis a complete series of transformations of the deposited matter may be found in different parts after death, ranging between typical tubercle and caseous matter, and it is apparent that, in place of uniform diffusion, there are successive eruptions of tubercular granulations in various viscera, or in various parts of the same viscus. The recurrent attacks of fever observed in tuberculosis bespeak the same phenomena. Nevertheless, it would be an error to deny the occurrence of cases in which only the grey semi-transparent tubercles are to be met with. But such cases are pretty well limited to tubercular deposit on serous membranes, accompanied by typhoid symptoms. So, too, in acute pulmonary tubercularisation it may happen that death is brought about by violent congestion and suffocation, before the deposit has had time to develop beyond its first stage.

In the secondary form, where acute tuberculosis supervenes upon previous deposits or chronic lesions, a like parallelism obtains between the ancient lesions and the newly-deposited granulations; and galloping phthisis and acute tubercularisation may progress simultaneously.



Indeed, the fact of the pre-existence of caseous masses in cases of acute tuberculosis gave rise to Bühl's hypothesis that the latter derived its origin from the former as a direct result. Although this opinion cannot be maintained, there is an intimate relation between the two, and in all cases of acute tuberculosis a special search should be made for some local caseous deposit. Further, the close association observed is explicable by the fact of tuberculosis being a constitutional malady, wherein multiple and repeated manifestations happen in the same individual; so that every one carried off by tuberculosis will, in all probability, have had at some previous time a local and non-febrile attack; and the old caseous masses represent simply the advanced stage of evolution which the new granulations deposited will undergo if only the life of the patient be sufficiently prolonged.

The general facts established, therefore, are—that in an anatomical and clinical point of view, the acute forms of tuberculosis depend on two different processes: in one, multiplication of the initial lesion, so that the grey granular substance proceeds, without definite limits, to at once invade, either in whole or in part, the parenchyma of various organs, both intra- and extra-thoracic; whilst, in the other, there is a rapid evolution of a lesion already constituted—the lesion of typical (classic) phthisis. However, these two processes are frequently associated together. Moreover, in so far as these two processes operate separately, they present (1), from the anatomical point of view, the common character of rapidly advancing the lesion towards softening, and the evacuation of the tuberculous matter; (2), from the clinical point of view, this common character, the exhibiting like general symptoms when they affect the same organ (the lungs), being undistinguishable, except by physical exploration, which permits the establishment of a distinction between them akin to that established between pneumonia and bronchitis.

After entering on the subject of prognosis in the two conditions, M. Colin details at length, with instructive comments, several cases in elucidation of the views he upholds.—*Archives Générales de Médecine*, June and July, 1874).

*Experimental Investigations on the Production of Cough.* By Dr. O. KOHTS.—Regarding the physiological inquiries hitherto made respecting the production of cough as inconclusive, Dr. Kohts instituted numerous experiments upon cats and dogs in elucidation of the subject. The general results arrived may be briefly summed up. Cough follows more rapidly from irritation of the larynx than of any other portion of the respiratory tract. The free borders of the vocal cord are themselves not sensitive, but if the sound or feather introduced do but touch the inter-arytenoid space or the glosso-epiglottic or the ary-epiglottic folds, a paroxysm of coughing supervenes. Excitation beneath these points provokes cough, but this of a less violent character. It is not clear that irritation of the true pulmonary tissue will cause cough; but this act follows as a constant result irritation of the mucous membrane of the trachea and bronchi, and especially at the bifurcation of the trachea. Again, irritation of the pleura induces coughing, but

neither constantly nor with facility. It acts most powerfully in the neighbourhood of the roots of the lungs, less in the pleura costalis, and not at all in the pleura pulmonalis. Although pathological observations indicate the production of cough by irritation of the pericardium, yet Dr. Kohts could not demonstrate the fact experimentally, on account probably of the difficulties attending the proceedings necessary to do so. Irritation of the mucous membrane of the pharynx is a cause of cough, but this only of a brief duration, and a repetition of the irritant will generally fail to induce it, owing to a voluntary act bringing about an effort of deglutition. But if the pharyngeal nerves themselves be acted on, cough in proportion to the irritation follows. If the external ears be tickled coughing is induced, owing probably to a branch derived from the vagus nerve. A rough and harsh cough ensues upon irritation of the œsophagus, and usually, before the act of coughing, a deep inspiration takes place, and the œsophagus gets distended with air. The cough was louder and stronger when the irritant was applied directly to the longitudinal muscular fibres, whilst to the mucous membrane itself the irritation failed. The experiments on the stomach failed to produce cough; nevertheless, clinical experience demonstrates the existence of a "stomach-cough" dependent on various causes of irritation in the stomach and duodenum, acting through the branches of the vagus. Some lesions of the liver may also give rise to a like phenomenon.

Irritation of the trunk and branches of the vagus of the superior laryngeal and the pharyngeal nerves, induces a hard, dry cough; but, in the case of the recurrent laryngeal, the results were negative. Experiments showed that direct irritation of the brain and spinal cord caused coughing. To the question of the existence of independent nerve-centres for inspiratory and expiratory movements, Dr. Kohts's experiments failed to give a decided answer, but he is disposed to believe that there is an expiratory centre, by which the act of coughing takes place, at a point somewhat higher in the medulla than that concerned in inspiratory muscular action.

An account of the experiments performed accompanies the paper from which the above extract has been made.—*Virchow's Archiv*, May, 1874.

A critical examination of this paper by Kohts is made in the 'Gazette Hebdomadaire,' by Dr. Lereboullet. Kohts states that if the superior laryngeal nerve be divided, cough fails to be produced by tickling the glosso- or the ary-epiglottic folds of the larynx, but that direct excitation of the centripetal fibres of the superior laryngeal nerve, and also of the trunk of the vagus and the filaments of the glosso-pharyngeal nerve, brings on at times an energetic cough. These statements are pointed out as opposed to the results attained by Nothnagel, who failed to induce cough by direct excitation of the nerve-trunk, a circumstance in harmony with the general observation that irritation of the peripheral branches of nerves is more productive of excited or reflex action than that of the trunks. Moreover, Rosenthal, to whom Kohts makes no reference, concluded the superior laryngeal nerve to be the special nerve of cough, having demonstrated that



excitation of the centripetal fibres of the superior laryngeal nerve—a nerve antagonistic to the pneumogastric, and consequently causing arrest of respiration, determined relaxation of the diaphragm, spasmodic occlusion of the glottis, and violent paroxysms of cough.

Kohts's experiments and conclusions with regard to cough in connection with irritation of the pleura, stomach, and uterus, were, it is contended, not calculated to evoke truthful results; for in them the pathological conditions which determine the occurrence of cough were ignored.—*Gazette Hebdomad.*, June 26, 1874.

*Overstrain of Heart.* By Prof. SEITZ.—This condition and its results have not met with the attention they deserve from writers on heart disease. Seitz has collected many examples of it and deduced its clinical history more completely than hitherto done. It is thus briefly traced. A man, still young, is admitted into the hospital with a history of good health generally prior to his present illness. He has worked hard, but is no longer able to do so, the slightest effort brings on dyspnœa and palpitation; he has a feeling of constriction across the chest, and the præcordia seems to be oppressed by a heavy weight. Any further efforts induce pains over the cardiac region, shiverings, cough, and hæmoptysis; sometimes also there is loss of consciousness. The tendency of the malady is to advance in each particular symptom; the patient is threatened with suffocation during the paroxysms, the face grows livid, the lower extremities become œdematous, and at times anasarca supervenes, with effusion into the serous sacs. The predominant feature is the dyspnœa, and everything points to heart disease; for, besides the symptoms narrated, the pulse is irregular, small, and feeble, the cardiac impulse weak, the præcordial dulness either normal or considerably increased, and the heart-sounds heavy and not clear. But usually there is no murmur; now and then a very slight souffle with the first sound is audible, and occasionally some slight rubbing may be detected. The diagnosis suggested is either insufficiency of the mitral valve or pericarditis.

In some cases, the state of the patient improves sufficiently to allow of his discharge and recommencing work, but at the end of a few weeks the former symptoms reappear, and he succumbs either suddenly or else from advancing asphyxia. The autopsy exhibits congestion of all the viscera, such as is met with in heart disease generally; the pericardium normal, the heart dilated, but with its walls of normal appearance and structure,—though possibly here and there a few fibres may be found degenerated, and the valves normal. Either among the columnæ carneæ or in the auricles are to be found some old coagula. There is, in short, a remarkable absence of organic changes that might fairly have been anticipated. The symptoms are indicative of cardiac weakness, but it is clear that such weakness is independent of appreciable lesion of structure. At the same time the circumstances under which the disorder made its appearance are those of overstrain of the heart by excessive work. The slight auriculo-ventricular souffle sometimes heard is attributable to a relative insufficiency of the orifice attending the dilatation of the heart. The condition above described

corresponds with that designated "le cœur forcé" by M. Beau, and also with the "asystolie" of M. Raynaud. The same condition has been referred to by several British physicians, particularly by some in the Army Medical Service; and, we may add, overexertion in boat-racing is one of its well-established causes. It will be well, however, to have its pathological significance put on a wider and more evident basis, both with the view of more correct diagnosis and treatment.—*Deutsches Archiv*, Band xi, xii, and *Révue des Sciences Médicales*, Tome iii, 1874.

*Chronic Hæmorrhagic Peritonitis*.—Prof. Friedreich details a case of chronic heart disease with general dropsy and ascites, in which tapping was resorted to sixteen times, between the 26th of September, 1869, and the 4th of February, 1871. On the last occasion the ascitic fluid was very decidedly bloody. This fact, coupled with the conditions found on examination after death, has furnished Friedreich with data for recognising a special form of chronic hæmorrhagic peritonitis.

The case was that of a woman, æt. 30, who had had four natural labours, and since her seventeenth year repeated attacks of acute rheumatism, complicated with marked narrowing of the mitral valve, and enormous dilatation of the tricuspid. Both surfaces of the peritoneum were found, after death, lined by a false membrane. The false membrane was everywhere of a yellow or of a deep brown colour, with scattered patches of recent and superficial hæmorrhage; but both in thickness and in depth of colour, that portion spread over the parietal layer of the peritoneum was most marked. In fact, this portion was separable into several laminae, and also presented numerous rather firm tumours, composed of altered blood effused between the layers of new tissue (hæmatomata). The new formation could be everywhere readily stripped from the subjacent peritoneum, which, although unduly vascular, exhibited its normal polish. No adhesions had occurred with the intestines. Examination of the brain showed the sinuses of the dura mater to be gorged with blood, and the dura mater itself over the convexity of the hemispheres to be lined by a delicate reddish-brown pellicle, readily detached, and showing scattered patches of recent hæmorrhage. The pia mater of the right posterior lobe was likewise infiltrated with ecchymoses, the ventricles distended with serum, and the cerebral substance hyperæmic and œdematous.

Of the neo-plastic membrane in the abdomen, the layers nearest the peritoneum were the oldest and thickest, the firmest and most organised. They consisted of fibrillar tissue, with a liberal supply of blood-vessels, and some free pigmentary masses of a yellowish or brownish colour. The addition of acetic acid brought to light numerous nuclei, having usually one nucleus, but in some instances two. The layers more recently formed were richer in vessels and in pigmentary matter, more delicate in texture and consistence, but yet of a fibrillated appearance. The latest deposited laminae readily broke up, displaying corpuscles of like character, though some of them were branched, and had oval nuclei, with one or two nucleoli. Some fatty granules and com-



mencing fibrils were also noticed. The appearance generally resembled that of certain sarcomata, or, still more, embryonic connective tissue. In other places the cell elements were larger, rounded, uninuclear, and among them were many showing a transition to stellate forms, enclosing fatty granules and pigmentary particles of a brown or yellow colour.

The most recent layers were remarkably alike by their pigmentation and vascularity. Their vessels were gorged, varicose, and ruptured in places, giving rise to extravasation. From these last, the blood colouring the ascitic fluid had evidently been derived; and of the same nature were also the hæmatomata in the older and denser layers.

The appearances narrated are, in Friedreich's opinion, conclusive of the existence of a special form of peritoneal inflammation heretofore little noticed. From the ordinary forms it is distinguishable by the production of superposed layers readily separable from one another, and from the peritoneum; by its slight tendency to form adhesions, and by its excessive vascularity, its hæmorrhages, and its pigmentary deposits. In its etiology it is, moreover, remarkable. In reference to this the frequent tapplings and their consequences must be considered. Each one is necessarily attended by a sudden diminution of pressure on the abdominal vessels, and particularly on those of the peritoneum, which, from a state of more or less previous compression, would be subjected to a fluxionary movement. In these conditions a course of irritation may be discovered leading to a genesis of proliferating tissue. The existence of such irritation was, indeed, shown by the occurrence of pain, increased on pressure, after each operation, and lasting several days. That the results were, however, not due to inflammatory action originating in the puncture is, at the same time, evident from the diffusion of the morbid action over both the parietal and visceral peritoneum. The appearance of blood only on the occasion of the last tapping may be explained by a determination and accumulation of blood in the newly formed layers, and by the progressive thinning and weakening of the vessels. Probably, also, the hæmatomata found were formed in the interval of the twelve days between the last puncture and death; their recent-looking features favour this view, although the rupture of vessels and extravasations may be regarded as natural consequences of the rapid accumulation of ascitic fluid after each tapping.

Friedreich takes occasion to draw a parallel between this form of peritonitis and the peculiar inflammation of the dura mater described by Virchow under the name of chronic hæmorrhagic pachymeningitis. In the one, as in the other lesion, there are repeated congestive attacks followed by contemporary formation of false membranes; and if the production of a hæmatoma of the dura mater be primarily associated with chronic hæmorrhagic pachymeningitis, it is even so with the production of a peritoneal hæmatoma. It is, moreover, worth notice as a peculiarity in the case recorded, that the chronic hæmorrhagic peritonitis was complicated by pachymeningitis of the same nature in its first stage, and without hæmatoma. The reddish-brown membrane

covering the inner surface of the dura mater proved to be composed of the elements of connective tissue of an early period, of an abundant development of vessels, and of numerous pigmentary masses, precisely like the most recent layers deposited on the peritoneum.

Collateral fluxions must have taken place to the brain when, from the rapid increase of ascites, the abdominal vessels were obstructed. So it happened that on each occasion that the ascites reached a certain amount the patient complained of throbbings, of weight of head, of sleeplessness, and of noises in the ears, and that all such symptoms vanished when the dropsical fluid was evacuated. By the recognition of this alternation of hyperæmia and congestion in the head and in the abdomen, it is possible to explain the concurrent phenomena of internal pachymeningitis and of an analogous form of inflammation of the peritoneum.

The only other lesions of the peritoneum assimilated to the one described are retro-uterine hæmatocele, or at least a particular form of it, and inflammation of the recto-vesical cul-de-sac in the male. In both these conditions an inflammatory process is set up, followed by false membranes and abnormal vascularity. Local irritations, often repeated as they are prone to be in the regions referred to, induce other fluxions, and with them extravasations ending in hæmatoma. In this way can be comprehended the analogy between hæmorrhagic peritonitis, such as described and retro-uterine hæmatocele on the one hand, and between the last-named lesion and hæmatoma of the dura mater, as demonstrated by Virchow, on the other.—*Archives Générales de Médecine*, June, 1874.

*Ammoniacal Urine and Urinary Fever.*—The object of this essay by MM. Gosselin and Albert Robin is to determine, experimentally, the cause of the fever and other accidents that ensue upon operations on the urinary passages. In fulfilling their task they examine severally, (1) the character and nature of the decomposition urine may undergo; (2) the general and local action of carbonate of ammonia dissolved in water only and also in urine; and (3) the action of ammoniacal urine. A fourth point, that concerning remedial measures, they postpone for consideration in a future memoir.

The first division of the essay goes only to demonstrate our ignorance of the nature and the modes of reaction of the components of urine undergoing decomposition. The only fact that seems clear is, that ammonia is separated. The second division of the memoir concerning the action of ammonia dissolved in water and in urine respectively, is dealt with experimentally. The results obtained with solutions of ammonia are on the whole confirmatory of those arrived at by other experimenters. As might be anticipated, they differ according to the strength of the solution, the rapidity of injection, and the quantity injected. When given in strong and large quantities, after prodromata of restlessness, cries, slight convulsive movements, loss of power in the hind legs, and from two to four tetanic attacks follow. Respiration is noisy, slow and deep; the pulse feeble and slow; epistaxis common; and a fall of temperature in direct relation to the poisonous



dose and its influence in producing tetanus. Coma, interrupted by more or fewer convulsive shocks, supervenes on the termination of the tetanic state; but, except the poisonous dose have been too large, recovery may follow. Among other phenomena are, albuminuria and an alteration of the blood, consisting in a diminution of globules and an augmentation of albuminoid matters, attended by a change of colour to a reddish brown and a loss of coagulability.

When the ammoniacal fluid is injected in feeble but repeated doses, the consequences observed are little marked. No convulsive or tetanic seizures ensue, and in place of reduced temperature there is an elevation, which is maintained within a certain range of oscillations, until the local effects become destructive of vitality. It is thus held to be demonstrated, that the repeated and continuous absorption into the system of ammonia though an abrasion or wound is not followed by symptoms of general poisoning, but only by local injuries, represented chiefly by topical suppuration. And further, it appears that the consequences of poisoning by a watery solution of ammonia do not bear any clinical resemblance to those of the absorption of urine as witnessed after operations on the urinary passages.

The next series of experiments was undertaken to determine the action of normal urine when injected, and the results arrived at were, that when the urine is injected in small but repeated doses, it is rapidly absorbed and its effects only slight and local. The thermometer showed some febrile action, of limited amount, and autopsies revealed no changes, saving some slight renal congestion.

On the other hand, when ammonia, even in small quantity, is added to urine, the noxious character of the mixture becomes very apparent and death an early consequence. Fever is set up in four or five days; the oscillations in temperature are diurnal, with a progressive upward movement; whilst defervescence is sudden and soon followed by death. The toxic power increases in direct proportion with the quantity of ammonia added. Albuminuria and visceral congestions appear, and the blood readily coagulates, even in vessels of medium size, unlike what happens with injections of simple aqueous solutions of ammonia, or with those of normal urine. Moreover, local abscesses more quickly form, together with purulent deposits. An examination of these phenomena exhibits a considerable relationship between them and those of urinary fever (*fièvre urineuse*).

The next stage of investigation was that of the effects of ammoniacal or decomposing urine; and the experimenters took pains to determine the alkalinity and the quantity of ammonia existing in the specimens of urine they employed. Their experiments proved such urine to be more noxious than the artificial mixture of urine and ammonia. The fever induced sprang up almost immediately after the first injections, and death quickly resulted. Accompanying the fever were noted also various symptoms; agitation, cries of pain, diarrhoea, loss of appetite, and towards the end, difficult noisy respiration, lateral decubitus and albuminuria. The febrile condition observed recalled very closely that attendant upon operations on the urinary passages. After each injection there was an immediate elevation of temperature, which after a

while decreased; to be presently revived by a fresh injection, until ultimately a continued febrile state was set up, terminating by sudden defervescence and death.

The addition of ammonia to the decomposing urine previously employed rendered it vastly more poisonous. Moreover, if instead of excluding air as was done in the injections already alluded to, air was admitted into the wounds to mix with the injected material, the consequences were still more fatal; apparently so from the more rapid decomposition set in motion, and the quicker evolution of ammonia.

One deduction from these experiments is, that in the case of an operation, where the urine is acid, two conditions are required to develop poisonous consequences, viz., the presence of air and the admixture of pus or of blood with the urine, as essential factors in its decomposition. Another is, that although the action of decomposing urine be more intense in proportion to the quantity of carbonate of ammonia it contains, yet that this last-named substance is not the sole factor in the production of the evils consequent upon the absorption of urine after operations; for a fetid specimen is more noxious than one not so, although the proportion of ammonia be alike in the two.

Lastly, as a matter of pure hypothesis, the authors of the essay suggest an analogy between urinary fever and septicæmia, and the possible influence of microscopic organisms introduced into the blood by the medium of decomposing urine.—*Archives Générales de Médecine*, May and June, 1874.

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## REPORT ON MATERIA MEDICA AND THERAPEUTICS.

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*On the Physiological and Therapeutical Properties of Saffron.* By Dr. DELIOUX DE SAVIGNAC.—Dr. de Savignac, in a somewhat elaborate paper, gives a complete history of saffron, and seems to think that as a medicine its efficacy is too little appreciated in the present day. As a medicine it was included in the 'Materia Medica' of Dioscorides, and was recommended by Hippocrates and Galen. Chemical analysis, however, does not reveal any important constituents of saffron, the chief components being colouring matter and an essential oil, to which latter probably any therapeutical or physiological effects it produces are attributable.

Although saffron was very much extolled as a medicine by the ancients, and subsequently by the Arabian physicians, and although it is still largely employed by the medical practitioners of all civilised



countries, it has lately been regarded only as a colouring and flavouring ingredient in various medicinal compounds. But Dr. de Savignac believes that it possesses some real efficacy, its properties being both stimulant and sedative. In some pathological conditions of the gastrointestinal tract it has been used with advantage, as in dyspepsia, flatulence, enteralgia, &c., in which it acts as a tonic, stomachic, carminative, and sedative. He also believes that it acts as an emmenagogue, for although this property is denied to it by some authorities, he has known it to induce the flow of the menses, or to render menstruation more normal when complicated by uterine spasm or violent pain. It appears to possess some properties analogous to those of castoreum, assafoetida, and valerian, in controlling the nervous disturbances which interfere with the functions of the uterus. On the whole, Dr. de Savignac considers saffron as a kind of aromatic bitter, rather tonic than exciting in small doses, but in large ones possessing the latter character and having sedative properties besides. It is peculiarly adapted for the treatment of some diseases of women and children, acting as a cordial and stomachic, and a sedative in pain and spasm. In the male subject, in order to obtain adequate results, it must be given in a large dose or in combination with other drugs.—*Bulletin Général de Thérapeutique*, May 15 and 30, 1874.

*On the Employment of Ipecacuanha administered in Injections in the Choleric Diarrhœa of young Children and in the Diarrhœa of Tuberculous Patients.* By Dr. CHOUPEE, of Paris.—Dr. Choupee divides his paper into three sections—1, the treatment of choleric diarrhœa in young children; 2, that of the diarrhœa of tuberculous patients; and 3, the mode of administration of the ipecacuanha in injection.

1. There were five cases of choleric diarrhœa in young children treated in the manner indicated, in four of which the result was quite successful and the cure was rapid; in the fifth, death ensued after a transient improvement. The cases were all serious, and occurred in September, 1873, when fatal cases of diarrhœa in children at the breast were numerous and when cholera evidently prevailed. In almost all the five cases recorded by Dr. Choupee other measures had been unsuccessfully resorted to before the employment of the ipecacuanha injections. It was found that in no case did this mode of treatment produce any gastric symptoms. From a consideration of all the cases, Dr. Choupee believes that ipecacuanha acts with great energy in the diarrhœa of young children, especially when administered in injections; that by this mode of administration the injurious effects of vomiting are avoided; and that the treatment may be continued for a long time without weakening the patients.

2. Diarrhœa is almost a constant symptom in tuberculous patients, but presents different characters at different periods and in different cases. The small intestine appears to be always the primary seat of the serous secretions, but when the diarrhœa is connected with ulceration the large intestine is sometimes affected, as Dr. Choupee has himself ascertained in many cases. In seventeen cases recorded there

were thirteen cures, in two the patients were relieved, and in two the treatment was unsuccessful.

3. The injections employed were made by taking twenty grammes (a gramme is about fifteen grains) of ipecacuanha root bruised, and 500 grammes of distilled water, and boiling the root three times in a portion of the water, mixing the products of the three decoctions and adding a little laudanum to each injection. In children the laudanum was omitted. Two injections were ordered to be administered every day; the first between 7 and 8 o'clock in the morning, namely, two hours before breakfast; and the second about 8 o'clock in the evening, or about three hours before the last meal. No vomiting ever occurred under this treatment, and only once was there some little nausea, which did not last long.

There is considerable difficulty in explaining the therapeutical action of ipecacuanha, and the examination of its chemical composition does not throw much light on the subject. Water takes up two active substances, namely, *emetine* and *tannin*; but an analysis of the solid matter contained in the injections, made under the superintendence of Dr. Chouppe, showed that in three grammes of dry residue there were .58 of a gramme of emetine and .09 of a gramme of tannin. This amount of tannin is so small that Dr. Chouppe finds it impossible to admit that the injections owe their astringency to this principle.

The conclusions drawn by Dr. Chouppe are the following:—1. Ipecacuanha administered in injections produces satisfactory results in the diarrhoea of tuberculous persons and in choleric diarrhoea in young children. 2. By this plan the gastric symptoms almost constantly attending the employment of ipecacuanha in the ordinary way are avoided. 3. The injections may be continued for a long time without producing either disturbance of the digestive functions or debility. 4. The drug, under the circumstances, appears to act by absorption.—*Bulletin Général de Thérapeutique*, June 15, 1874.

*Researches on the Eucalyptus Globulus as a Febrifuge and Expecto-  
rant.* By Dr. H. OEFFINGER.—In this paper the negative results obtained by some physicians in treating intermittent fevers by the use of the eucalyptus are brought into relation with the application of a variety called *Eucalyptus latifolius*, instead of the leaves of *E. longifolius*. The dried leaves are always more uncertain than the fresh ones and the tincture prepared from the latter. It is only when the eucalyptus is to be given as an expectorant that Dr. Oeffinger uses the infusion of the dried leaves, but in all other cases he employs the tincture. Dr. Oeffinger and Dr. Müller, of Efringen, have tried the latter in intermittent fevers independently of one another, and have arrived at corresponding results, namely, that by the employment of from 60 to 80 grammes an antipyretic effect is produced as a general rule, but sometimes 120 grammes are necessary. The diminution of splenic tumour was often perceptible as a result of this treatment. Dr. Oeffinger treated thirty-seven cases of intermittent fever in the above manner, and cured thirty-one without relapse, and six with relapse; five remained uncured; and of these two were improved by quinia. He



ordered 30 grammes respectively of tincture of eucalyptus, aqua menthæ, and simple syrup, and ordered a tablespoonful of the mixture to be taken every two hours. The temperature of the patients was reduced from 41·0° C. to 36·6°. In one case the treatment was continued for sixteen days, and in another neither the use of the eucalyptus for fourteen days nor the administration of quinia cured the disease. Dr. Oeffinger is convinced of the efficacy of the new remedy, and thinks that his researches prove the fact, and he draws some other conclusions, namely, that the eucalyptus is sometimes useful where quinia fails; that, as a rule, quinia fails where the tincture of eucalyptus has failed; and that the latter drug leaves no unpleasant after-consequences.—*Schmidt's Jahrbücher der Gesamnten Medicin*, May 12, 1874.

*On the Treatment of Phthisis by the Phosphate of Lime and the Juice of Raw Meat.* By Dr. HENRY BLANC.—Dr. Blanc states that the juice of raw meat and the phosphate of lime combined have given him some very good results in severe cases of phthisis. In France three preparations of phosphate of lime are employed, and they are called respectively the lacto-phosphate, the chlorhydro-phosphate, and the monocalcic phosphate; but Dr. Blanc does not seem to think that any one of them possesses any decided advantage over the others. The treatment of phthisis by raw meat and alcohol was advocated some years ago by Professor Fuster, of Montpellier, and at first the results were exceedingly favorable; but eventually it was found that the patients could not be induced, after a certain time, to swallow the raw meat or the stomach to tolerate it. Dr. Blanc attributes this failure to the improper mode of administering it, and he proposes a plan which he has found successful. He presses out the juice from the meat by means of a powerful meat-press, and this juice is mixed with equal parts of tepid broth, made of bones and flavoured with salt and pepper, and to which tapioca or vermicelli may be added. Dr. Blanc lays down precise rules as to the times when the food should be given, and when the raw-meat juice and the phosphate of lime should be added. Warm milk with bread and butter, and fat bacon and eggs, form the first and early meal, and the breakfast, at 11 or 12 o'clock, should be preceded by a dose of the syrup of the triple phosphate of lime, and accompanied by the raw-meat juice and a dose of the muriatic phosphate of lime. The dinner is at 6 o'clock, consisting, like the breakfast, of fish, or white meats, or poultry, fresh vegetables, and a few glasses of good alcoholic wine, and should be accompanied by some more of the raw-meat juice, and, instead of the triple phosphate, a dessert-spoonful of cod-liver oil may be taken afterwards. The muriatic solution or wine of phosphate of lime should also be taken during the dinner. Dr. Blanc advises that no medicines beyond those mentioned should be given, unless some special indications or urgent symptoms demand their use. He states that his cases so treated all did well, although the last winter in Paris (where the cases were observed by him) was very damp and the climate variable.—*Lancet*, June 13, 1874.

*On the Treatment of Tetanus by Chloral.*—In a recent discussion at the Société de Chirurgie of Paris, Dr. Chauvel, of Havre, related two cases of tetanus treated unsuccessfully by chloral, but Dr. Verneuil stated that, although in the beginning of his practice he had never seen a case of tetanus cured, he had had five successful cases since he employed the drug in question. He also mentioned two cases successfully treated by Dr. Richelot by means of chloral. In one of these the tetanus was caused by a violent effort made by a carpenter in raising a piece of wood, but there was no wound; in the other case there was a superficial wound of the head. In the first case, the medium dose of chloral was six grammes a day (a gramme is about fifteen grains), and the treatment lasted twenty-two days; in the second the medium dose was ten grammes a day, and the duration of the cure was one month. Dr. Verneuil, in commenting on these cases, admits that the first was not strictly speaking traumatic, and that the second was a chronic case; but, nevertheless, he considers them both instances of true tetanus. As to the mode of administration, the ingestion by the mouth is the best; but sometimes the patient cannot swallow, and then the drug must be introduced in some other way; and in extreme cases Dr. Verneuil thought that it might be advantageously injected into the veins. Dr. Tillaux, at the same meeting, stated that he had that very morning successfully treated a female affected with tetanus by injecting chloral into the veins. The attack followed upon the opening of an hydatid cyst of the liver. Dr. Boinet also stated that he had treated four cases of tetanus with chloral during the siege of Paris, and two were cured; he was very much in favour, therefore, of the treatment proposed. Dr. Lefort, while admitting that chloral induces rest and sleep, and causes the contractions of tetanus to cease, maintained that it does not cure the disease. Dr. Verneuil, however, argued that if chloral produced sleep and relaxed the spasm of the muscles in tetanus, it was not worth while arguing whether the drug alleviated the symptoms or cured the disease, any more than it would be to inquire whether mercury cures syphilitic symptoms but does not cure syphilis. He himself had cured cases of tetanus by chloral, and he therefore advocated that mode of treatment, which, he believed, caused a cessation of the spasms, just as in epileptiform convulsions it caused those convulsions to cease, and thereby prevented the consequent lesions of the spinal cord.—*Bulletin Général de Thérapeutique*, May and June, 1874.

*On an Effervescent Water containing the Tribasic Phosphate of Lime.* By Dr. CHEVRIER, of Paris.—The therapeutical use of phosphate of lime has been limited for several years to a very small number of pharmaceutical preparations, and as the failure of the phosphate in therapeutical power seemed to be due to its insoluble or difficultly soluble form, various proposals have been made for administering it in combination with other bodies so as to make it enter into solution in the alimentary canal. Thus MM. René, Blache, and Dusart, published in 1869 and 1870 some researches on the effects of the phosphate dissolved in lactic acid, and they showed the beneficial effects of the



preparation on the increase and consolidation of the bones as well as on the general health in many cases. The success which followed this plan of administration led other experimenters to substitute hydrochloric for lactic acid as a solvent of the phosphatic salt. Dr. Chevrier shows, however, that, by employing either of these acids on the phosphate, a mixture of *acid phosphate of lime* and another *calcium salt*, are produced by the chemical reaction induced, and he has therefore sought for an agent which, without decomposing, will dissolve the tribasic phosphate of lime, the substance required by the economy. His experiments have shown that the phosphate may be dissolved in carbonic acid, and that its solubility increases with the pressure. Thus a glass of effervescent water can dissolve and retain a quarter of a gramme (a gramme is about fifteen grains) of the tribasic phosphate, a dose which Dr. Chevrier thinks sufficient, and which, besides, may be easily repeated several times a day. The mode of preparing this effervescent solution is simply to pass the carbonic acid into a gelatinous mixture of the phosphate, and, after allowing the excess of phosphate to be deposited, the clear solution is to be decanted into bottles such as those used for holding the natural mineral waters. Dr. Chevrier thinks that this effervescent form of administering the phosphate is superior to the methods hitherto in use, inasmuch as it presents the tribasic calcium salt without any mixture of other salts, and he says that the preparation may be mixed, without decomposition, with wine, beer, or milk, and may therefore be given with the meals, which is a very favorable condition of assimilation.—*L'Union Médicale*, July 16, 1874.

*The Subcutaneous Injection of Calomel employed as a Method of Diagnosis in a doubtful Case of Syphilis.* By Dr. DOMENICO STEFFANINI, of Pavia.—A girl, of thirteen, who had never had any previous illness, complained of a sore-throat, which, however, did not give her much uneasiness, and the medical attendant who saw her thought the case a slight one, and prescribed an astringent gargle. This application was soon proved to be useless, and the patient found her voice becoming more and more husky and nasal every day, and the liquids taken by the mouth were rejected by the nostrils. She at last presented herself as a patient at the Civil Hospital of Pavia, and it was found that the velum palati was jagged at the edges, the uvula was destroyed, and in its place were two small excrescences covered with healthy mucous membrane, the tonsils were a little swollen, and each presented a broad ulceration of a dirty grey colour, with hard and fringed edges, the whole of the mucous membrane was swollen and hyperæmiated, and the glands of the neck were engorged. Dr. Steffanini suspected the disease to be specific, and accordingly made inquiries of the patient and her mother, but the former had clearly never had any primary symptoms (among other proofs the hymen was intact), and the mother denied that she or her husband had syphilis. The absence of the ordinary proofs of specific disease induced Dr. Steffanini first to resort to local remedies, such as chlorate of potash, nitrate of silver, alum, &c., but as this treatment, after a fortnight's trial, proved ineffectual,

it was determined to employ, as a method of diagnosis, a subcutaneous injection of calomel, in the dose of three centigrammes dissolved in a gramme of glycerine. In two days afterwards there was a diminution of the smarting in the throat on swallowing, and the ulceration assumed a better aspect. In about a week more the ulcerations of the tonsils were cured. A second and a third subcutaneous injection were subsequently made, with decided improvement in all the symptoms, and among others, in an attack of iritis which developed itself. For this last affection a solution of atropia was also employed. The tuberculous excrescences which had replaced the uvula gradually disappeared entirely, some small abscesses which had formed were opened, the smarting in the throat was removed, and eventually, after a fourth injection, the patient appeared to be completely cured. The treatment lasted from the 31st of January to the 3rd of April.—*Giornale Italiano delle malattie venerei e della pelle*, April, 1874.

*On the Therapeutical Effects of the Iodides and Bromides of Mercury.* By Dr. BELLINI, of Florence.—In this paper, which is one of a series relating to the therapeutical effects of the whole of the salts of mercury, Dr. Bellini investigates the changes produced in the alimentary canal on the iodides and bromides of mercury, and he examines successively the changes produced on the protiodide and protobromide, and on the biniodide and bibromide.

It had already been ascertained by Mialhe that the protiodide and protobromide of mercury, when submitted to the action of the alkaline chlorides in presence of the air, were converted into corrosive sublimate, which was produced in greater quantity when the mixture was exposed to a high temperature. But Dr. Bellini doubts whether the reactions produced in the alimentary canal on these mercurial salts ought to be limited to the effects of the chlorides and hydrochloric acid, for he has shown that the gastric juice converts the protiodide and protobromide of mercury into soluble compounds of the metal, which then descend into the intestines and are subjected to the action of the alkaline carbonates. He believes that he has proved by his researches that the protiodide and protobromide are dissolved (though in different proportion) by the stomach when fasting, but that, when taken into the stomach with the food, if the aliments are chiefly of the proteinaceous kind, the salts are all decomposed and reduced for the most part to the state of metallic mercury, and only in a very small proportion to the state of soluble compounds. Dr. Bellini has experimentally placed the above-named salts of mercury in contact with the albumen of the egg, muscle, bullock's liver, lamb's sweetbread, animal gelatine, leguminous plants, &c., and he has found that the mercurials were reduced to the metallic state, and that in this reaction a soluble compound of mercury was found in a certain quantity, as was proved by the addition of sulphuretted hydrogen. He also investigated the effects of certain other agents, as acid drinks, fruits, &c., on the activity of the mercurial salts, and he found, by experiment, that by pouring citric acid, tartaric acid, and acetic acid, in a diluted state, on the protiodide and protobromide, and keeping the mixture for some



hours at the temperature of the animal body, a soluble compound was formed only with the protobromide. Hence he concludes that when acid drinks or fruit are administered together with this salt they should increase its action by rendering it more soluble, and thus promoting its therapeutical effects.

In reference to the changes effected on the biniodide and bibromide of mercury, Dr. Bellini has found that when placed in contact with lactic and hydrochloric acid and the alkaline chlorides, in the proportion in which these substances exist in the gastric juice, the mercurials in question form also soluble compounds, and that both are affected in nearly the same manner.

The general results at which Dr. Bellini has arrived from his researches may be summed up as follows. The iodides and bromides of mercury are converted more or less in the alimentary canal into double salts. The reagents which cause this change in the stomach are the alkaline chlorides, the lactic and hydrochloric acids, and the proteinnaceous aliments, animal and vegetable; and in the small intestines, besides the alkaline chlorides, there are alkaline carbonates of the intestinal fluids. The reagents in question do not act with the same energy on the iodides and bromides of mercury, forming double salts, for a smaller quantity of these is produced with the protiodide than with the protobromide, and a less quantity even with this than with the biniodide and bibromide. In the large intestine the iodides and bromides of mercury and the double salts formed by their decomposition remain free and unchanged in sucking children, while in children not sucking and in adults they are converted into sulphides by the sulphuretted hydrogen in this part of the intestine. The iodides and bromides of mercury, applied externally either to healthy or diseased parts, or injected into the subcutaneous areolar tissue, are partly converted into double salts chiefly by the action of the alkaline chlorides which they meet with. The iodides and bromides of mercury act locally as well as generally, owing to their conversion into double salts. The iodides and bromides of mercury, although in somewhat different degrees, undergo pretty nearly the same changes as calomel does in the animal organism. Sulphur and the alkaline hyposulphites, being taken into the system together with the iodides and bromides of mercury, counteract the operation of the latter by the development of sulphuretted hydrogen which is then produced in the whole of the alimentary tract.—*Lo Sperimentale. Firenze, April, 1874.*

[The paper of Dr. Bellini, of which the above is necessarily a brief abstract, will be seen to throw a very great and important light on the therapeutical action of the mercurial iodides and bromides, and of the mercurial preparations in general.—REPORTER.]

*On the Use of the Alcoholic Extract of Nux Vomica in large Doses in various Forms of Nervous Diseases, both acute and chronic.* By Dr. DE STEFANI.—Dr. de Stefani combats the opinion of those physicians who regard nux vomica as an irritant of the spinal cord and explain its beneficial action in paralysis on this assumption, his own experience having demonstrated to him that the drug exerts a depressing action

on the ganglionic system. As this system has numerous relations and sympathies with the cerebro-spinal, the nux vomica, according to Dr. de Stefani, acting upon both, relaxes the vital tension of the nerves, restores to them their natural conducting power, and also the degree of influence necessary to maintain the harmony of the vital functions of the organs, and consequently all the alterations in the vital fluids dependent on nervous affections are mitigated and removed even without the aid of any other medicines. From this belief in the depressing effects of nux vomica Dr. de Stefani employs it, and especially its alcoholic extract, in all acute and severe hypersthenic maladies, as well as in chronic ones, whenever any nervous symptoms are manifested in connection with the ganglionic or cerebro-spinal system. In acute diseases, in proportion to the largeness of the dose, the curative action is more rapid, so that in twenty-four hours the patient may be rescued from danger in most instances. The action of nux vomica on the ganglionic system is marked only by alleviation of the morbid symptoms, or at most by a slight tremulousness of the heart and internal viscera, but on the cerebro-spinal system it is manifested by a feeling of confusion (*sbalordimento*) in the head, and of something, as it were, penetrating into all the fibres to the marrow of the bones, and sometimes by a vivacity of the senses, or a tremulousness of the whole body or some slight spasm of the facial muscles. The tolerance of the drug is not always equal. In acute and serious diseases of the two nervous systems the tolerance is great, in the chronic affections of the ganglionic system it is greater than in that of the cerebro-spinal, and in the organic diseases it is in relation to the gravity of the nervous sympathies. Dr. de Stefani recognises the intolerance of the drug by a test which he considers infallible. By the rapidity of its action and the great sympathy between the two nervous systems, and by the extensive range of the spinal symptoms, the intolerance is so indicated in these last by the stiffness of the lower jaw and of the tongue, with some degree of subsultus in the lower limbs or in all the body, and when these indications are present a warning is conveyed as to the effects of the medicine.

Dr. de Stefani maintains, in reference to the curative action of nux vomica, that the drug depresses the muscular force, if this has been stimulated by hypersthenia, and stimulates it when it has been apparently depressed by the same cause; that it lowers the pulse when it is hard and vibrating, and raises it when it is small and weak; that it lowers excessive heat of the skin and warms the skin when it is morbidly cold; that it regulates both the pulse and the heat of skin when they are variable several times in the day; that it relieves ardent thirst; that in costiveness which has resisted repeated purgatives it opens the bowels, and in some cases arrests diarrhœa; that it also arrests spontaneous hæmorrhage and relieves hæmorrhoids; that it relaxes spasms, removes neuralgic, pleuritic, and rheumatic pains, calms delirium, and removes morbid wakefulness, or awakes patients from morbid sleep, promotes perspiration when deficient or arrests it when profuse, &c., whenever these symptoms are the results of a nervous affection. Dr. de Stefani, however, gives a judicious warning not to employ the nux



vomica at first, even in cases where its use is indicated, unless the occasion be very urgent, because many nervous diseases may be cured by ordinary remedies. But when other means have failed, and the use of nux vomica is decided upon, then it is necessary to guard against giving too small doses, because the inefficacy of the dose might induce a doubt as to the action of the remedy, or, on the other hand, the supervention of new symptoms might be erroneously attributed to the drug instead of to the disease itself. When therefore the first prescription does not produce the desired effect, the dose should be increased as long as tolerance exists, and should be repeated for two, three, or more days, and, even when the symptoms have been relieved, the medicine should be continued, in order to prevent a relapse.

The dose of the alcoholic extract of nux vomica recommended by Dr. de Stefani in subjects of middle age suffering from chronic disease is from five to ten centigrammes (about  $\frac{1}{20}$  to  $\frac{1}{10}$  of 15·432 grains), and in serious cases this dose may be raised to 14 to 30 centigrammes, combining it with an equal quantity of extract of rhus radicans and some extract of henbane. In very severe diseases Dr. de Stefani thinks that this dose might be doubled or trebled, as, for instance, in tetanus and hydrophobia, but he has not yet employed it in such cases.

Dr. de Stefani gives the history of twelve cases treated by him in the manner indicated, and he regards them as fully proving the efficacy of the drug.—*Lo Sperimentale*, Firenze, May and June, 1874.

*On the Use of Quinine in the Diseases of Children, especially in Febrile Affections and Hooping-cough.* By Dr. RAPMUND.—Quinine and cold-water affusion both possess very certain and energetic antipyretic properties, and Dr. Rapmund in the present paper shows that quinine may be successfully employed in the diseases of infancy to a greater extent than is generally done. His observations were made on cases of a very tender age, and some of the patients were still at the breast. The maladies in which he employed the quinine were scarlatina, measles, smallpox, erysipelas, pneumonia, follicular enteritis, and hooping-cough. In the febrile affections he found that after the administration of the alkaloid the temperature and the frequency of the pulse rapidly fell, and the children sank into a tranquil sleep. He also thought that quinine exercised a marked influence on the evolution of the disease, which became mild in its progress, and when the fever was beginning to return, a fresh dose of quinine immediately checked it. In the lobular pneumonia of infants Dr. Rapmund found quinine particularly efficacious, and of nine cases treated, which were of ages from four months to a year and a half, only two died. In typhoid fever in very young children Dr. Rapmund has not yet employed quinine, but in the cases in which he used it, the children being upwards of six years old, he observed the most satisfactory results. In three cases of follicular enteritis the treatment by quinine was satisfactory, but great attention to diet was necessary, and one of the patients died. In hooping-cough the use of quinine was beneficial, for although the duration of the malady was not abridged, yet the

number and violence of the paroxysms were considerably diminished, and the nights became more tranquil. In reference to the mode of administration Dr. Rapmund has always employed a solution of the hydrochlorate of quinine in whooping-cough, the dose being from one to five centigrammes (a centigramme is the  $\frac{1}{100}$  of about fifteen grains) twice a day. The ordinary solution of hydrochlorate of quinine being very bitter, he gives it in a solution of glycerine and water, which, when added to a small quantity of strong coffee (*café noir*), loses its bitterness, and is easily taken by the child. When it could not be taken by the mouth Dr. Rapmund prescribed it in injection.—*Deutsches Klinik*, 1874.

*On the Inhalation of Balsamic Substances.* By Professor LEOPOLD DITTEL.—The well-known fact that the urine of persons who inhabit rooms freshly covered with oil of turpentine soon smells of violets, as if the oil had been taken internally, led Dr. Dittel to employ the inhalation of ethereal oils in the treatment of catarrh of the pelvis of the kidney. He simplified the injection apparatus very considerably, and the air impregnated with the volatilised ethereal oil was inhaled by a mouthpiece communicating with a bottle containing the drug. Dr. Dittel relates several cases in proof of the efficacy of this mode of treatment. In the first, an old man, there was frequent micturition, pain, and sleeplessness. After other remedies had been employed without any benefit, the patient was ordered to inhale twice a day the ethereal oil of pine for a month, but during this time the treatment was interrupted five times by attacks of irritation in the vas deferens. The quantity of urine varied from 1400 to 2435 cubic centimètres, and the specific gravity between 1.009 and 1.016; the reaction was twice neutral, but at other times acid. The absolute amount of sediment fell from 40 cubic centimètres to 17.5, and albumen gradually disappeared. Instead of making water twenty-eight times a day and more, the patient at last did so only four times, and eventually there was improvement in the sleep, the nutrition, and the bodily strength, and he was discharged essentially improved in health. In a second case the number of times of making water fell from seven to three; in a third case a cure was effected after sixteen inhalations, and the patient was improved even at the first inhalation. In a very obstinate case of cysto-pyelitis the beneficial effects of the plan described were very well marked. The patient had been treated by catheterism, but his condition became much worse, and there were fever, night-sweats, œdema of the feet, bloody urine, and great sleeplessness. Morphia, tannin, alum, acetate of lead, quinine, lithia, and various mineral waters were tried in vain, but at last the inhalations were employed, and with such success that the patient completely recovered.—*Schmidt's Jahrbucher der Gesammten Medicin*, May 12, 1874.

*On a Case of Traumatic Tetanus successfully treated by Calabar Bean.* By JOHN CUNNINGHAM, M.B., Campbeltown.—The case was that of a boy, aged 7, who had met with an accident affecting the great toe and first and second metatarsal bones, and the wound was



lacerated and contused. The injury was inflicted on the 31st of July 1873, and, under ordinary surgical treatment, it appeared to be doing well until August 11th, when the patient complained of a sore throat, the muscles on the back of his neck became stiff, and he was fretful and peevish, and refused food. A mixture of bromide of potassium and hydrate of chloral was ordered, but was not taken, as the friends of the patient thought the case was hopeless. On the 12th of August the condition was the same, but on the 13th it was determined that some medicine should be administered, and Mr. Cunningham ordered twenty drops (equivalent to  $\frac{1}{24}$  of a grain) of a solution of fresh extract of Calabar bean to be given every hour and a half, and also sherry, milk, and chicken soup, at frequent intervals. The patient, however, grew worse, and the tetanic spasms were more violent. Twenty-five drops of the solution were now ordered to be taken every hour. On the 14th the patient was slightly worse, but he had taken wine, milk, and soup abundantly. Thirty drops of the solution were now given every hour. On the 15th the symptoms were still worse, and thirty drops of the solution were again ordered every hour. In the evening of this day there was a slight improvement, but on the 16th and 17th the condition was again worse, and thirty-five drops and then forty drops of the solution were given every hour. On the 18th there was some improvement and a little sleep, but as this improvement was not maintained, on the 19th fifty drops of the solution were given every hour, and from this day the improvement was rapid and permanent, the spasms becoming fewer and less violent. The dose of the solution was gradually reduced from day to day, and the patient eventually was quite restored to health.

Mr. Jalland, of York, in commenting on this case, in a subsequent number of the Journal in which Mr. Cunningham's case appeared, attaches considerable importance to the state of the pupils as an indication of the effect of the Calabar bean on the system, and he mentions a case which he saw where the bean was successfully employed in tetanus, and where the relaxation of the spasm was coincident with the contraction of the pupils, the spasms returning when the drug was left off and the pupils resumed their normal state. —*British Medical Journal*, April 4th and 11th, 1874.

*On the Therapeutical Effects of Alcohol, as deduced from Experiments.* By Professor BINZ, of Bonn.—In this paper Dr. Binz examines, by the test of actual experiment, the question whether alcohol increases or diminishes the heat of the body, and therefore, whether it is a suitable remedy in febrile diseases. He comes to the conclusion that the heat is diminished by alcohol, his experiments having been made both on the lower animals and on man. He administered a certain quantity of pure alcohol mixed only with tepid water and sugar to a strong healthy man suffering from a joint disease without inflammation. This was administered daily for seven days, and the temperature was accurately noted, and it was also noted during seven days in which no alcohol was given. The result, as shown by curves, was that the administration of the alcohol caused a most

remarkable diminution of temperature. Experiments on rabbits led to the same result, but in that case the animals were placed in an artificially feverish condition by having some ichor or putrefying blood injected under their skin. Under these circumstances a condition is caused resembling typhoid fever in the human subject, and the animal expires in a few days, but this event does not ensue if, simultaneously with the experiment, alcohol diluted with water is administered either by the stomach or under the skin. The analysis of such experiments shows Dr. Binz a threefold action produced by the alcohol: (1) the diminution of the heat of the body; (2) reduction of the putrid processes; and (3) increase of the action of the heart. Thus it is proved that the supposed rise of temperature of the blood by means of alcohol has no existence, and that the apparent heat in such a case, as well as the apparent cold in an aguish patient, are only subjective phenomena. Alcohol, again, may be expected to diminish the metamorphosis of the tissues, because, if it lowers the combustion, it may be supposed to decrease the urea and the carbonic acid, and the researches of several authors prove that such is the case.

As to the employment of alcohol in fever, Dr. Binz calls attention to the fact that its effect is not so strong and lasting as that of other antipyretic agents. Large and repeated doses are necessary in order to maintain the lowering of the temperature; but, on the other hand, there are cases, as, for instance, when the heart is very weak, in which alcohol acts as an antipyretic while quinine is powerless. In all feverish illnesses the first thing required is to suppress high temperature, and if this is accomplished the physician removes the greatest danger, moderates the progress of the disease, and gives the organism the possibility of resisting successfully the internal cause of the malady. In speaking of alcohol, Dr. Binz expressly states that his observations apply only to pure preparations of that fluid, although all good alcoholic liquids, the percentage of which is known, may serve for common use. But he protests against the bad artificial and unpurified mixtures which are often given. The nauseous smell they impart to the breath indicates, he says, their injurious composition, for pure alcohol gives no taint to the breath, and good alcoholic liquids only impart the smell which belongs to their ethers.—*Journal of Anatomy and Physiology*, Second Series, No. XIV, May, 1874.

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## REPORT ON OBSTETRICS, GYNÆKOLOGY, AND PÆDIATRICS.

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### I.—THE NON-PREGNANT STATE.

1. *The Innervation of the Uterus.* By Professor CYON.
2. *Rupture of the Vagina and Protrusion of the Intestines.* By Dr. FEHLING.
3. *Primary and Isolated Cancer of the Uterus.* By Professor SPIEGELBERG.
4. *Incision of the Cervix Uteri.* By OLSHAUSEN.
5. *Solid Tumours of the Ovary.* By Dr. LEOPOLD.
6. *Use of the Actual Cautery in Uterine Disease.* By Dr. LEBLOND.
7. *Exudations around the Female Genital Canal.* By Professor SPIEGELBERG.
8. *Successful Ovariectomy in a Child eight years of age.* By Mr. SPENCER WELLS.

1. The author communicates the results of experiments made on dogs and rabbits of different ages; some under the influence of curare, others not. The chief results arrived at were the following:—

1. The uterine plexus is the most important, if not the only, motor nerve which can produce effectual movements of the uterus by the irritation of its peripheral ends (irritation of the cerebral ends only gives rise to severe vomiting). 2. Irritation of the cerebral ends of the first two sacral nerves produces reflexly powerful uterine contractions, which cease after the uterine plexus has been cut through (irritation of the peripheral nerves gives rise to powerful contractions of the bladder and rectum). 3. Irritation of the brachial, crural, median, sciatic nerve, &c., gives rise to no peristaltic movements of the uterus, but only causes a slight rigidity and paleness. 4. The effect of the irritation of these nerves disappears if the aorta, has been previously compressed; but irritation of the cerebral ends of the sacral nerves still causes, even after closure of the aorta peristaltic movements of the uterus. 5. Suffocation through the continued interruption of respiration causes powerful peristaltic movements, probably through direct excitation of the involuntary muscular fibres by the accumulated carbonic acid.—*Pflüger's Archiv*, Bd. viii, Heft 6 and 7, 1874.

2. The author was called to a woman, aged 63, who had suffered from prolapsus uteri for thirty years. He found her in a state of collapse, conscious, and complaining of pain in the abdomen. Some hours before, she had carried a pail of water up some steps and the

womb prolapsed; she tried to replace it and used some force; she felt something give way and the intestine protruded. Dr. Fehling found the coils of the small intestine protruding at the vulva, forming a mass as large as a man's head. By vaginal examination the intestine was followed into the abdominal cavity through a large rent in the posterior wall. An attempt was made to replace the bowel, but it was unsuccessful; puncturing was then tried, but with no good result. The patient died eleven hours after the accident, from shock. At the autopsy there was no sign of peritoneal inflammation. Dr. Fehling has found no reference to any case of rupture of the vagina occurring under such circumstances.—*Archiv für Gynäk.*, Bd. vi, Heft 1.

3. Prof. Spiegelberg relates a case of this rare affection occurring in a woman, aged 49, who had never borne children. She had had a constant sanguineous discharge for a year and accompanied with severe pains. The uterus was uniformly enlarged. The cervix was dilated by sponge tents, and on passing in the finger the posterior wall was found to be infiltrated and covered with breaking-down masses. These growths were scraped off by a curette, and there was but little hæmorrhage. After a few days the symptoms returned, and dilatation was again resorted to, and the curette was used to scrape off any prominence remaining. Soon after, the woman became collapsed and died with symptoms of peritonitis. At the autopsy it was found that the greater part of the posterior wall had been destroyed, and in the anterior wall there was a funnel-shaped opening into the abdominal cavity.—*Archiv f. Gynäk.*, Bd. vi, Heft 1.

4. The author recommends the performance of this operation in those cases in which, through the narrowness of the external os, dysmenorrhœa or sterility, or both, exist. When the narrowing is at the internal os, or produced by a flexion or a version, dilatation, without incision, should be employed. In some cases of sterility this operation is judiciously performed for its removal. The incision is best performed with Marion Sims' knife. The cut should be made from above downwards, first on one side and then on the other. To secure a good result the incision should be made high enough up the cervix, and the vaginal portion should be completely cut through. Means are to be taken to prevent the wound healing too quickly, either by placing dilating stems in the canal, laminaria or sponge tents, which latter are dangerous, or, better, by breaking down adhesions by the finger and the use of the sound. If ante flexion and sterility exist together the operation must be modified by cutting through the anterior lip, or even removing a wedge-shaped piece. The operation may be usefully performed in severe cases of catarrh, where, through the narrowness of the external os, the secretions are retained; also this permits intra-uterine medications to be more easily applied. At the time of the operation no inflammatory complications should exist.—*Sammlung Klinischer Vorträge*, No. 67, 1874.

5. Solid tumours of the ovary are rare. The author, from the facts



he has collected, estimates the proportion as 1·5 per cent. of fluid tumours. He has collected forty-three cases from various sources, and has added thirteen not published before. Solid tumours may be distinguished externally from fluid tumours by their shape; the former retain the natural shape of the ovary, the latter are irregularly rounded. The consistence varies they may be so soft as to give indistinct fluctuation, or as hard as a stone. The thickness of the external wall varies much, and this the author thinks important as regards the more or less rapid development of the tumours, according to the resistance which it offers. The anatomical relations are much the same as with the fluid tumours. These tumours have at times a short pedicle, and are to be detected between the uterus and the rectum, and may be held immovably fixed there. Such tumours, if carcinomatous, &c., very rarely affect the uterus, which, as a rule, remains perfectly healthy.

Tumours of the ovary may be fibromas, enchondromas, carcinoma, or sarcoma. The fibromas are simple or complex, fibro-myoma or fibro-sarcoma. Two other rarer forms may be added to these, one described by Waldeyer, the other by Spiegelberg. Enchondroma of the ovary is quite exceptional. Sarcoma is supposed to be very rare in the ovary, and little mention is made of it in text-books on gynecology. The author, who has specially worked at the subject, gives a full account of them. He then describes a previously undescribed form of ovarian tumour, which he calls "lymplangioma kystomatosum." It is characterised by the cystic formation, the dilatation of lymphatic vessels, and a proliferation of the stroma. The author directs attention to the condition of menstruation in women suffering from cysts or solid tumours of the ovaries. We see that menstruation may go on even when the two ovaries are degenerated, and further cases have been reported where menstruation has returned in women who have had both the ovaries removed. From such facts as these he inclines to the belief that menstruation has no direct relation with ovulation. Ovariectomy has been performed eight times for these solid tumours, and in three cases the operation was successful. The Cæsarian section was once obliged to be performed on a woman whose pelvis was blocked by a fibroma ossified in part.—*Archiv für Gynäk.*, Bd. vi, Heft 2.

6. In a lecture on this subject the author gives the following as the conclusions he has arrived at:—1. That the disease for the treatment of which the actual cautery is best indicated is chronic metritis, whether accompanied or not by ulcerations. 2. That in chronic metritis the cauterization should be profound or superficial; profound in the first period or period of congestion, superficial in the second or period of anæmia. 3. That in cancer the actual cautery is rarely useful, and that, on the contrary, it may become very dangerous. 4. That acute inflammation of the uterine parenchyma, and above all a peri-uterine inflammation, formally contra-indicates the use of the hot iron, and that pregnancy should make one very cautious in employing it. 5. That the use of the actual cautery is nearly absolutely

innocuous, provided always that one takes care to follow the advice given as to its application.—*Annales de Gynécologie*, 1874.

7. A knowledge of this branch of gynækology is of recent date, and is due chiefly to French authors, Marchal de Calvi, Nonat, and Bernutz and Goupil. Two opposite views of the etiology of these exudations have been propounded: one regards them as inflammations of the serous membrane covering the genital organs, pelvi-peritonitis being the ruling disorder; the other as phlegmons of the pelvis, the connective tissue surrounding the genital canal and forming the parenchyma of the broad ligament being the seat of the disease. Whether peri- or para-metritis is the more frequent is difficult to say, and there is much to be learned respecting them. Spiegelberg thinks our ignorance is mainly due to deficient knowledge of the precise anatomical relations of the serous layer and pelvic connective tissue on the one hand, and of the relation of the latter to the uterus and vagina on the other. He endeavours to elucidate the subject, and gives an account of certain anatomical observations illustrated by drawings. He dwells on the importance of what he calls the parametran tissue, a layer of tissue containing large blood-vessels, venous plexuses, lymphatic glands, and nerves, surrounding the lower section of the uterus and base of the vagina, for inflammation of which he proposes the name "parametran inflammation," a term he prefers to parametritis. The inflammation of the great mass of the pelvic connective tissue forming the broad ligaments, and reaching from them to the rectum and sacrum and anterior abdominal wall, he would call, with the French authors, phlegmon of the broad ligament or of the pelvic connective tissue respectively. Parametran inflammations arise from diseased states of the lower part of the internal genital organs, whether following labour or the result of injuries, as from violent jerks, excessive coitus, cauterization, incision, amputation, or dilatation of the cervix, &c., or arising from cold or disease about the cervix. Phlegmon of the broad ligament is rather due to extension of a parametritis, or it starts in the tissue around the ovary. Pelvi-peritonitis accompanies affections of the body of the uterus and the Fallopian tubes, especially those of the internal layer. The latter are much rarer than the former, more frequently of a secondary nature; their course is never so lingering as that of an inflammation of the connective tissue.—*Volkmann's Sammlung*, 1874.

8. Mr. Wells relates a very interesting case of ovariectomy in a child eight years of age. "A free cyst was tapped, emptied, and drawn out with a solid mass at the base. There was a long pedicle, and I was able to pass a silk ligature and tie the pedicle in two portions without including the Fallopian tube. I cut the ends of the silk close to the knots, leaving very little tissue beyond the ligatures on cutting away the tumour. The tied pedicle and the knots of the ligature were allowed to fall back into the pelvis. On examining the uterus and other ovary with one finger I was doubtful which ovary I had removed, though I believed it was the left. The uterus



did not feel as large as a walnut, and I could not feel an ovary nor the ligature I had just applied. The wound was closed by silk ligatures passed in the mode I usually practise in ovariectomy. During the passage of the sutures there was more difficulty than usual in preventing escape of intestine and omentum, but there was no other peculiarity in the operation. A little ascitic fluid came away after the cyst. The solid portion of the tumour weighed three ounces, and the cyst contained twenty ounces of fluid. In the solid portion there was a mass of bone covered with true skin, from which grew a quantity of long light hair. The hair was rolled into balls and matted together by sebaceous matter. The skin covering the bone was perfect and lay on a bed of adipose and areolar tissue. The bone was proved on section to be true bone, not a mere calcareous degeneration. . . . During the first and second days after the operation the little patient had some pain and was rather feverish. After the third day recovery was uninterrupted, and she sailed from Liverpool for New York twenty-five days after ovariectomy."—*British Medical Journal*.

## II.—THE PREGNANT STATE.

1. *Thermometry of the Uterus.* By Dr. SCHLESINGER.
2. *Sulphate of Quinine as an Abortifacient and Oxytocic.* By Dr. CHIARI.
3. *Remarks on the Course and Treatment of Labour in the Contracted Pelvis.* By Professor SPIEGELBERG.
4. *Puerperal Pyæmia, &c.* By Dr. MATTHEWS DUNCAN.
5. *On Prolapse of the Umbilical Cord; its Cause and Treatment.* By Dr. GEORGE J. ENGELMANN.
6. *The Pelvis in Double Dislocation of the Hip-joint, &c.* By Dr. KLEINWÄCHTER.
7. *The Sacrum, according to Sex and Race.* By Dr. BACARISSE.

1. The author has made a number of experiments upon non-pregnant women, and has ascertained that the temperature of the unimpregnated uterus exceeds by  $0.19^{\circ}$  C. on the average that of the vagina. This he explains by the muscular work of the organ. This fact takes away all the value, as far as diagnosis is concerned, from the difference of temperature which certain authors have found in the pregnant uterus and the vagina, and which they hoped to employ as a means of diagnosis in a difficult case of pregnancy in the early months, or to establish the vitality of the foetus.—*Wien. Med. Wochensch.*, p. 215.

2. The author has given quinine to forty patients in the Royal Catherine Institution of Milan, and has come to the following conclusions as to its effects in such cases:

- (1) The disulphate of quinine has no action as an abortifacient.
- (2) Quinine cannot be trusted, either alone or in conjunction with mechanical means, for the induction of premature labour.

(3) In cases of slow, suspended, or irregular labour, it is not well to trust to the action of quinine.

(4) The assertion of Ponti, of Parma, that ergot must give way to quinine is chimerical, at least as regards midwifery.

(5) When quinine is indicated by the presence of general morbid conditions during pregnancy, it should be given, not only as a remedy for the disease, but also as the best means of preventing abortion or premature labour.

(6) Quinine has no power whatever in preventing or modifying morbid conditions of the puerperal state, whether of infectious or of sporadic origin.—*Gazetta della Cliniche*, No. 29, 1873.

3. The author, in a paper on this subject, says there are three forms of contracted pelvis which we often meet in practice. viz.—1. The simple flat pelvis (rickety or not). 2. The uniformly and generally contracted pelvis. 3. The generally contracted and flat pelvis.

In the first form it is the anterior part of the head which is engaged at the inlet; the chin is easily separated from the breast; the large fontanelle comes lower down than the small, and the coronal suture approaches the conjugate. The head is freed thus—the sagittal suture comes more forward, the occiput moves more backwards. Labour here may end spontaneously. If interference is required the forceps can only exceptionally be used. As a rule, the head must be lessened. If the difficulty is recognised sufficiently early we should have recourse to turning.

In the second form, where the pelvis is generally contracted, the head meets an obstacle in all its circumference as it enters the pelvis. The head is strongly flexed, and may be represented by a wedge, the occiput is presenting, and the face is directed to the fundus of the uterus. Here the hinder part of the head is first engaged. Here impaction of the head is most to be dreaded, and we must often have recourse to craniotomy.

In the third form we have the anterior or the posterior part of the head first engaged, according as the flattening or the general contraction predominates. At times this may bring about the presentation of the head in a position inclined to one side, and we have “a presentation of the ear.” If such a presentation is detected in time turning should be performed; if not, we must perforate.

The treatment in contracted pelvis may be thus enunciated:—It will often be necessary to perforate; that version should be reserved for certain fixed cases, and that the use of the forceps should be rejected.—*Archiv für Gynäk.*, Bd. iv, Heft 2.

4. Dr. Duncan, in his very able address on obstetric medicine before the British Medical Association, at Norwich, treats this subject in his usual philosophic manner. As a necessary preliminary, Dr. Duncan refers to “the mortality in childbed, or total mortality of childbirth and in childbed.” He says, “The ordinary belief seems to be that there is, in connection with childbirth and lying-in, no mortality in a well-conducted practice;” and further on, respecting



such statements, remarks, "We have no time to trifle with such nonsense, for we are everywhere surrounded by awful deaths in childbirth and in childbed, where there has been, so far as can be discovered by ordinary mortals, nothing but proper conduct and management."

Dr. Duncan clearly shows that, although unhappily we are at present unable from lack of the necessary data to estimate with any reliable degree of exactness the true mortality of childbirth and childbed, yet that there is an amount of mortality which may be regarded as constant, if not inevitable. (We may say *en passant* that the whole world is under obligation to Dr. Duncan for his past able work on this subject, to which the present is a fresh and valuable contribution.)

Dr. Duncan estimates, from the latest data he has procured—50,000 births in Edinburgh and Glasgow in 1869 and 1870—that the mortality of lying-in women within four weeks is 1 in 120; and believing that the mortality of puerperal women does not fall to its ordinary level till a period, not of weeks, but of months after delivery, he concludes that in this country nearly 1 in every 100 women delivered at or near the full time dies in parturition or before the puerperal state and its effects have passed away.

Deaths during parturition or the puerperal state are often conveniently arranged in three sets:

1. Childbirth deaths.
2. Puerperal or metria deaths.
3. Accidental deaths.

The second in this list is the one of greatest importance, and it is to it that Dr. Duncan devotes the chief portion of his address. He advises the rejection of the term "puerperal fever," as embodying and perpetuating an erroneous belief in the essential nature of the disease.

He proposes, until the advance of science displaces it by a better, the name puerperal pyæmia, but gives a caution against the use of the latter word as implying purulent blood. "That was once the meaning of pyæmia, but it is not so now."

The term is to be used as a comprehensive word, identical with or including the septicæmia or ichorrhæmia of certain others. Dr. Duncan combats the old notion of the term "puerperal fever," implying, as Dr. Fordyce Barker (the Duncan of America, as Dr. Marion Sims happily calls him) holds, the idea of "an essential fever peculiar to women, as much a distinct disease as typhus or typhoid." On the contrary, he believes that "all the evidence brings the disease into the closest alliance or identity with surgical pyæmia," and this is the key-note of his address. He denies that epidemics of it ever occur, and states that it follows a very different law to smallpox or cholera; its ravages vary, as those of pneumonia.

Regarding the disease as pyæmic in character, Dr. Duncan logically advises that the precautions now so commonly taken by surgeons should in like manner be observed by the accoucheur, and

that the practice based on antiseptic principles should be applied to delivery.

This is obviously more difficult than in ordinary surgical practice ; nevertheless, it would be well if Dr. Duncan's philosophic caution should be as fully observed as the circumstances of obstetric practice will permit.

5. Dr. Engelmann has contributed an able and instructive paper on this subject, and his conclusions, based on 160 cases with pelvic measurements, are as follows :

"The causes of the prolapse of the umbilical cord have mainly proved to be such circumstances as prevent the complete filling of the pelvic brim and the close adaptation of the lower segment of the uterus to the presenting part. One of the more important of these circumstances is the shape of the presenting foetal part itself, and we thus find that foot presentations are most frequently complicated by prolapse, whereas vertex presentations are least threatened.

"The foetal appendages are of secondary and minor importance ; undue length of the cord, its marginal insertion, or attachment of the placenta low down in the uterus, can never be direct cause of the accident ; excess of liquor amnii is alone to be feared.

"Some stress is to be laid on abnormality in shape and position of the womb, much more upon twin births. More dangerous than any of these is the contracted pelvis, which I have proved by measurements and numbers to be the main cause of prolapse of the funis, directly and indirectly, a fact hitherto generally accepted, but never as yet clearly established. Another such vague general statement, that the prolapse is by far more frequent among multiparæ than among primiparæ, our cases disprove ; they show that primiparæ are, comparatively speaking, almost as frequently afflicted as multiparæ.

"The law governing the location of the prolapse is of importance, and here for the first time touched upon ; it will, I trust, be verified by the investigation of other observers.

"The post-mortem examinations revealed only lesions due to death from asphyxia, nothing characteristic for death caused by prolapse of the cord.

"The prognosis we can give is somewhat better than generally allowed ; most favorable for foot presentations, after these for shoulder and transverse presentations, while vertex presentations are more dangerous than any ; the case being, under all circumstances, more threatening when occurring in a primipara.

"In the treatment of our cases the high importance of the postural method has been developed, more as an adjuvant, however, than as a method in itself dealing with the prolapse.

"Version is comparatively the most successful of all operations, and should be more frequently resorted to when any choice of method is given, as in head presentations ; the forceps and reposition of the cord are less to be relied upon ; but whatever may be the course determined upon, it must be borne in mind that the success of all



operations by which we seek the preservation of the child, whose life is threatened by compression of the prolapsed cord, is in a measure dependent upon the judicious use of chloroform, its application to full surgical anæsthesia."—*The American Journal of Obstetrics, &c.*, vols. vi and vii, 1874.

6. The first pelvis described by Dr. Kleinwächter resembles much the rickety pelvis flattened from before and behind; the second, from a woman seventy-one years of age, resembled more osteomalacic pelves, but both presented some characteristic peculiarities due to the mechanism of the deformity.

One may always, according to the author, assign the changes of form and position of the pelvis in cases of double luxation to two causes—displacement posteriorly of the centre of gravity, and modification of the action of the muscles in relation with the pelvis or which are inserted into it, the one undergoing stronger tension, the other being relaxed. These causes produce, first, a considerable inclination (about  $90^{\circ}$ ) of the pelvis, with augmentation of the curves of the vertebral column; and, secondly, an antero-posterior flattening of the pelvis, with uniform increase of the transverse diameters and diminution of the antero-posterior diameter, more marked at the superior inlet than at the lower.

During pregnancy the abnormal curves of the vertebral column are exaggerated; it is, therefore, more difficult for the patients afflicted with this infirmity to preserve their equilibrium during walking or standing. The pelvis undergoes a still more considerable inclination, and ultimately the extent of its oscillations during walking predisposes to accidental causes of abortion. As regards delivery, the pelvis of luxation may be likened to flattened rickety pelves, where the principal difficulty is the passage of the head in the superior inlet; the only obstacle which exists then is produced by the enormous inclination of the pelvis, and is easily remedied by placing the patient on her side.

The statistics of some cases, when one has seen pregnancy or delivery occur in analogous cases, appear to confirm these theoretical deductions.

In nine pregnancies there were two abortions and one premature confinement.

In the six other cases in which the pregnancy proceeded to full term the child was born alive in five cases.—*Vierteljahrschrift f. d. Prakt. Heilkunde de Prague*, vol. 118, p. 163.

7. M. Bacarisse first considers the principal peculiarities which the sacrum may present. He dwells particularly on the sacrum composed of six vertebræ; in this abnormal form, which he has met with more than fifty times in 146 specimens examined (more often in the male than in the female), the superadded piece is a *transitional vertebra* between the lumbar and sacral layers (and not a coccygeal vertebra); so the base of the sacrum is then raised between the iliac bones. It presents frequent anomalies (existence on the superior aspect of the sacrum, and on one side only of an articular facet on

the posterior part, a promontory existing between the first and second piece of the sacrum, &c.).

He then gives the results of a long series of measurements of the sacrum, examined according to race and sex. The following are the principal conclusions :

1. In all races, except in the negro races, all the dimensions of the sacrum in men, with one exception, are larger than the corresponding dimensions of the female sacrum. The dimension which is the exception to this rule is the size of the superior inlet, which is always greater in the female than in the male.

In all races the sacrum of man is more strongly curved than that of woman.

2. The size of the sacrum, at its base, attains its maximum in the white races, and in particular in Europeans; the curve of the sacrum is equally greatest in the white races. It is in the negro races that one meets with the flattest sacra.

Some of these conclusions differ completely from those which were enounced by Dr. Joulin in his work on 'The Pelvis according to Race.' For Dr. Joulin there was no difference between the pelvises of the yellow races and those of the negro races; as regards the pelvis, only two principal races could be established—the white races, and then, combined, the yellow and negro races.—*Thèse de Paris*, 1873, No. 437.

### III.—PÆDIATRICS.

#### (Diseases of Children.)

1. *On the Alteration of the Weight of Mature New-born Children.* By Dr. KÉZMÁROZKY.
2. *On the Prodromal Stage of Chorea.* By Dr. SCHMIDT.
3. *On the Use of the Actual Cautery in Prolapse of the Rectum in Young Children.* By M. PANAS.
4. *Case of Granular Contracted Kidney (Cirrhosis) in a Child under Six Years of Age.* By Dr. W. H. BARLOW.
5. *Contribution to the Study of Diphtheric Paralysis.* By Dr. KARL KETLI.

1. After giving a number of examples, Dr. Kézmározký gives the following conclusions as the result of his observations :

- (1) All children lose weight the first few days after birth.
- (2) The loss takes place in the first few hours after birth, but then for a time the weight may remain the same if an abundant supply of food be given before the intestines and bladder have been emptied; exceptionally there may be even an increase of weight, which, however, does not last beyond the sixth hour of life.

(3) The increase in the weight begins, as a rule, on the second or third day.

(4) The loss is more sudden than the gain, so that up till the



seventh day the gain has been scarcely more than half the previous loss.

(5) The beginning of the increase of weight has no connection with the separation of the stump of the umbilical cord.

(6) Boys begin to increase in weight on the average earlier than girls, they probably lose slightly less, and show greater gain than the latter; also a larger number of boys reach their original weight in the same period of time.

(7) The growth is more marked in children of pluriparæ than those of primiparæ.—*Archiv für Gynäk.*, Band V, Theil iii.

2. Dr. Schmidt says that this stage usually escapes the notice of the physician, as in the majority of cases he is not consulted until the malady has become pronounced. The prodromal stage is characterised by disturbances which chiefly arise from spinal irritation, and are confirmatory of the view held by Dr. Betz, that chorea is an affection of the central nervous system, especially of the spinal cord and its membranes. Pain is evinced on pressure of the spinous processes of the vertebræ, especially in the lumbar and dorsal regions. Complaints are made of rheumatic pains in the shoulder and neck, and, less frequently, in the head. The nose and anus itch, leading to the suspicion that the patient may have ascarides. The cardiac nerves show signs of irritation; there is inability to read or to fix the eyes for long, also flashes before the eyes. Sleeplessness and dreams by night and terror by day are frequent symptoms. Such symptoms Dr. Schmidt regards as indicative of anæmia depending on tuberculosis, scrofula, deficient nutrition, or the advent of menstruation.

Dr. Schmidt combats the anæmia by iron and tonics, and rubs the back with an ointment of oxide of zinc and opium.

3. M. Panas regards this affection as a very rebellious one, and recommends cauterization around the margin of the anus. He first covers the parts with a layer of collodion, to preserve them from the radiating action of the heat, but gives a caution to see that this layer is quite dry before using the cautery, or the collodion may catch fire. Several heated points, made of a platinum-pointed cautery, are then to be applied to the margin of the anus and in the thickness of the skin. The cauteries should be sufficiently heated.—*Journal de Médecine et de Chirurgie*, March, 1874.

4. Dr. Barlow relates an interesting case of this affection, which is remarkable on account of the rarity with which the disease is observed in children.

It was first noticed that the patient, a little girl, wetted the bed, and from the amount of urine passed and from the incontinence by day as well as by night it was thought that the child had diabetes. Her appetite was capricious. At first the quantity of urine passed in the twenty-four hours amounted to three or four pints, and albumen was not detected, though searched for; but subsequently, when the amount of urine diminished, albumen was found in large quan-

tity. The child had repeated convulsions, and ultimately died comatose.

There was never anasarca in any part of the body. At the post-mortem examination the kidneys were found to be very small and granular. There was complete absence of œdema.—*Lancet*, August 1st, 1874.

5. The paralysis which succeeds diphtheric angina is not always proportional to the intensity of the diphtheria. Sometimes a slight diphtheria produces extensive paralyses, and *vice versâ*.

Paralysis may occur during or after the diphtheric inflammation. It most frequently attacks the muscles of the velum palati first; sometimes, again, it appears in other parts, and only attacks the velum palati later, which it even avoids in some instances, as has been shown by the observations published by Prof. Bókai ('*Orvosi hetilap*,' 1863), under the title of "Ascending Paralysis of Childhood."

After the velum palati the muscles of the eye are most frequently attacked by paralysis.

It follows from the researches of Eulenberg that the sphincter of the iris and the tensor of the choroid are usually the first paralysed. Besides, the motor muscles of the eyeball may be alternately affected, the paralysis striking, for example, the internal rectus one day and the external rectus on the morrow, or conversely. This form of alternating paralysis is characteristic of diphtheria.

As regards diphtheric amaurosis, admitted by some authors, and in particular by Frerichs, Dr. Ketli contests its reality. According to him the cases which have been thus described have been for the most part badly observed, or confounded with the muscular paralysis of the eye.

When the paralysis invades the muscles of the extremities or of the trunk the patients experience sensations of numbness and weakness in the parts attacked, which increase more and more until the paralysis becomes complete. This paralysis is most frequently exclusively motor; still, sometimes it simultaneously affects sensibility. Frerichs and Gerhardt have, however, reported cases of anæsthesia pure and simple. When the motor paralysis of the extremities is complicated with a certain degree of anæsthesia, certain symptoms of ataxy may be produced, the resemblance of which to some of the phenomena of tabes has led some observers into error, and have induced them to erroneously admit the existence of a diphtheric tabes.

The paralysis of the extremities is generally symmetrical, and is accompanied neither by pain nor fever. The electro-muscular contractility is at first preserved, and is only weakened after a, generally, considerable time. According to the recent researches of Gerhardt, electrical sensibility may be diminished in some nerves towards the periphery, and remain, on the contrary, normal near the centre. As regards the temperature of the paralysed members, out



of four cases reported by Bókai, it was normal in three and slightly diminished in the fourth.

The evolution of diphtheric paralysis is generally rapid and favorable. In favorable cases the muscles resume their contractility and force in a few days; in others the paralysis may remain for several weeks; more rarely it remains for months. In certain, happily less frequent, cases the paralysis assumes a graver form, and tends to become general, the patients then dying of asphyxia from powerlessness of the respiratory muscles.

The pathogeny of diphtheric paralysis is still the subject of numerous hypotheses, because pathological anatomy has hitherto furnished only negative results.

Bretonneau's opinion, says the author, is no longer accepted; that of Trousseau, shared by Frerichs and more recently by Bókai, that the cause of paralysis is a diphtheric poisoning of the blood, hardly explains the special localisation of the paralytic process. Remak refers it to an alteration in the great cervical sympathetic. Weber, and with him Eulenberg, admit a degeneration of the nerves proceeding from the diphtheric centre, and following a centripetal direction; this, starting in the spinal cord, may irradiate in the different nervous spheres. Weber compares this process to that of tetanus proceeding from a peripheric lesion. Senator likens these paralysees to those which come on after dysentery, or which declare themselves in the course of the puerperal state. According to him there is not, to speak exactly, a specific diphtheric paralysis, but a simple propagation of the inflammation of the mucous membrane to the nervous ramifications comprised in the diphtheric centre, and from thence, it may be, to peripheric parts or to nervous centres (neuritis migrans, Leyden, ascending neuritis). Slight cases get well with almost no treatment; in the rebellious forms electricity, baths, shampooing, and tonics, may be successfully employed.

According to Eulenberg the paralysed muscles are sometimes attacked by a very rapid atrophy, which may resemble progressive muscular atrophy.—*Jahrb. f. Kinderheilk.*, vii Jarhg, 1 Heft, pp. 60-65.

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#### CORRIGENDA.

Page 49, line 12, for "Monat" read "Mouat."

" 179, line 5, for "Canty" read "Cauty."

" 180, line 6 from bottom, for "Canty" read "Cauty."

" 274, line 8, for "two" read "too."

" 248, line 24 from top, for "internal" read "altered."



# INDEX TO VOL LIV

## BRITISH AND FOREIGN MEDICO-CHIRURGICAL REVIEW.

|  | PAGE |   | PAGE     |
|--|------|---|----------|
| Abortion and death from vaginal injection of arsenic . . . . .         | 218  | Bell's 'Manual of Surgical Operations' . . . . .                    | 176      |
| Adulteration Act . . . . .   | 306  | Beni-Barde on hydropathy . . . . .                                  | 328      |
| Albert on temperature of heart . . . . .                               | 255  | Bile-spectrum, Dalton on the . . . . .                              | 259      |
| Alcohol, therapeutical effects of . . . . .                            | 511  | Binz on therapeutics of alcohol . . . . .                           | 511      |
| Alcoholic extract of nux vomica, uses of . . . . .                     | 507  | Birkett's 'Thompson's Conspectus' . . . . .                         | 424      |
| Alimentation, Cyr on . . . . .   | 306  | Bladder, passage of hairs from . . . . .                            | 234      |
| Allen, lectures on aural catarrh . . . . .                             | 400  | Bladder, eversion of, congenital . . . . .                          | 217      |
| American Rebellion, surgery of . . . . .                               | 65   | Blanc on raw meat in phthisis . . . . .                             | 503      |
| Ammoniacal urine and urinary fever . . . . .                           | 498  | Blood in urine, detection of . . . . .                              | 225      |
| Anæmia perniciosa, history of . . . . .                                | 487  | Blood-putrefying, toxicological properties of . . . . .             | 242      |
| 'Anatomist's Vade Mecum,' Wilson's . . . . .                           | 167  | Blood-stains, detection of . . . . .                                | 249      |
| Anchylosis of elbow-joint, new operation for . . . . .                 | 239  | Body and mind, by Maudsley . . . . .                                | 269      |
| Andrews, psychology of scepticism . . . . .                            | 412  | Boéchat on structure of thyroid . . . . .                           | 258      |
| Anderson's statistics of puerperal inflammations . . . . .             | 214  | Bohm on use of cold water in fever . . . . .                        | 328      |
| Aniline, a colouring matter for sausages . . . . .                     | 253  | Bonafont on diseases of the ear . . . . .                           | 396      |
| — red, poisoning by . . . . .  | 219  | Bonsdorff on treatment of croup . . . . .                           | 214      |
| Antiseptic surgery (review) . . . . .                                  | 1    | Books received for review . . . . .                                 | 266, 526 |
| Apoplexy, Lidell on . . . . .  | 87   | Both on smallpox . . . . .  | 292      |
| — pathology of (review) . . . . .                                      | 87   | Bozeman on varicocele . . . . .                                     | 237      |
| Arlidge, report on medicine . . . . .                                  | 487  | Braidwood on management of children . . . . .                       | 174      |
| Armitage on hydropathy in acute disease . . . . .                      | 328  | Brains, microcephalic, on . . . . .                                 | 81       |
| Armstrong's 'Introduction to Organic Chemistry' . . . . .              | 152  | Brain, congestion of . . . . .                                      | 87       |
| Arnold on lymph-canalicular system . . . . .                           | 254  | Brambilla on treatment of orchitis . . . . .                        | 234      |
| Arthritis, chronic rheumatic . . . . .                                 | 211  | Braun on gastric secretion . . . . .                                | 259      |
| — deformans . . . . .  | 211  | Bromide of mercury, uses of . . . . .                               | 506      |
| Asp on termination of nerves of salivary glands . . . . .              | 212  | Brown, Harvey, on quarantine on Gulf Coasts . . . . .               | 30       |
| Assurance, life, Sieveking on . . . . .                                | 163  | Brown-Séguard on communications between retina and brain . . . . .  | 264      |
| Atrophy, muscular, progressive . . . . .                               | 414  | Brunton on poison of venomous snakes . . . . .                      | 248      |
| Aural catarrh, Allen on . . . . .                                      | 400  | Bucknill and Tuke's 'Psychological Medicine' . . . . .              | 115      |
| — surgery, Hinton on . . . . .   | 397  | Budd, W., on nature of typhoid fever . . . . .                      | 367      |
| Autobiographical recollections, by Clarke . . . . .                    | 426  | Buren, Van, on surgical diseases of genito-urinary organs . . . . . | 357      |
| Bacarisse on sacrum according to sex and age . . . . .                 | 521  | Caisson disease, the . . . . .                                      | 253      |
| Bain on mind and body . . . . .  | 269  | Calabar bean in treatment of tetanus . . . . .                      | 510      |
| Bakewell on pathology of smallpox . . . . .                            | 292  | Callender on treatment of wounds . . . . .                          | 1        |
| Balantidium coli, cases of . . . . .                                   | 225  | Calomel, subcutaneous use of . . . . .                              | 505      |
| Balfour on heart diseases . . . . .                                    | 425  | Cancer of uterus . . . . .  | 514      |
| Balsamic substances, inhalation of . . . . .                           | 510  | Cannes and its climate . . . . .                                    | 168      |
| Barlow on cirrhotic kidney in a child . . . . .                        | 523  | Cauty on the diseases of skin . . . . .                             | 179      |
| Barker, Fordyce, on puerperal diseases (review) . . . . .              | 318  | Carcinomatous cystoma of testicle . . . . .                         | 218      |
| Barnes on medical and surgical history of American Rebellion . . . . . | 65   | Carpenter's 'Principles of Mental Physiology' . . . . .             | 269      |
| Beard on legal responsibility in old age . . . . .                     | 411  | Cardiac nerves, Schiff on . . . . .                                 | 264      |
| Bellini on therapeutics of iodides and bromides of mercury . . . . .   | 506  | Cataract in infants, by Rossander . . . . .                         | 212      |
|  |      | Catarrh, aural, lectures on, by Allen . . . . .                     | 400      |
|  |      | Cauterization in hæmorrhoids . . . . .                              | 236      |
|  |      | Cautery in prolapse of rectum . . . . .                             | 523      |

|                                       | PAGE     |                                       | PAGE |
|---------------------------------------|----------|---------------------------------------|------|
| Chemistry, recent works on . . .      | 152      | 'Dictionary of Medicine and Sur-      |      |
| ——— Rodwell's birth of . . .          | 156      | gery' (French) . . . . .              | 172  |
| ——— of carbon compounds . . .         | 156      | Diphtheritic paralysis, Ketli on .    | 524  |
| Chevrier on water with phosphate      |          | Disease, water treatment of (review)  | 328  |
| of lime . . . . .                     | 504      | Diseases of children, by Meigs and    |      |
| Chiari on quinine as an abortifaciant | 517      | Pepper . . . . .                      | 172  |
| Child, cirrhotic kidney in a . . .    | 523      | ——— infancy, by West . . . . .        | 159  |
| Children's diarrhœa, use of ipecac-   |          | Disinfectants, Letheby on . . . .     | 252  |
| uanha in . . . . .                    | 501      | Dittel on inhalation of balsamic      |      |
| Children, diseases of, Meigs and      |          | substances . . . . .                  | 510  |
| Pepper on . . . . .                   | 172      | Douglas on prevention of smallpox     | 292  |
| ——— report on . . . . .               | 522      | Drachmann on arthritis deformans      | 211  |
| ——— diseases, use of quinine in       | 509      | Drysdale on life history of monads    | 265  |
| ——— management of, by Braid-          |          | Dublin Obstetrical Society's 'Pro-    |      |
| wood . . . . .                        | 172      | ceedings' . . . . .                   | 169  |
| ——— new-born, weight of . . . .       | 522      | Duncan on puerperal pyæmia . . .      | 518  |
| Chloral, use of, in tetanus . . . .   | 504      | Ear, on diseases of the, by Roosa .   | 394  |
| Cholera, epidemics of, in France .    | 193      | —— works on diseases of the           |      |
| ——— and quarantine (review) . .       | 30       | (review) . . . . .                    | 394  |
| Chorea, prodromata of . . . . .       | 523      | —— on diseases of the, by Bonnafent   | 396  |
| Chouppe on ipecacuanha in diar-       |          | Earth, use of, in surgery . . . . .   | 1    |
| rhœa . . . . .                        | 501      | Edinburgh Asylum report . . . .       | 253  |
| Church's 'Laboratory Guide' . . .     | 158      | Elbow-joint ankylosis, new opera-     |      |
| Chvostek on tuberculosis of spinal    |          | tion for . . . . .                    | 239  |
| cord . . . . .                        | 489      | Electricity in capillary bronchitis . | 222  |
| Circumcision a means of conveying     |          | Emetine, toxicological action of .    | 247  |
| syphilis . . . . .                    | 251      | Emergencies, surgical, by Swain .     | 402  |
| Clarke's 'Autobiographical Recol-     |          | Endocarditis, malignant cases of .    | 221  |
| lections' . . . . .                   | 426      | Endoscope, use of, in treatment of    |      |
| Clementi and Thin on putrefying       |          | gonorrhœa . . . . .                   | 217  |
| blood . . . . .                       | 242      | Energy, conservation of . . . . .     | 134  |
| Clère on the men of the Commune       | 17       | Engelmann on prolapse of umbili-      |      |
| Colin on tuberculosis . . . . .       | 490      | cal cord . . . . .                    | 519  |
| —— on smallpox . . . . .              | 292      | Epidemics of cholera in France . .    | 193  |
| —— on quarantine . . . . .            | 30       | ——— smallpox in Dublin . . . .        | 292  |
| Collineau on political commotions     |          | ——— Stockholm . . . . .               | 229  |
| and insanity . . . . .                | 17       | Epidemiology, by Parkin . . . . .     | 292  |
| Communism, psychology of . . . .      | 17       | Epistaxis, compression of facial      |      |
| Conjunctiva, granular disease of .    | 429      | artery in . . . . .                   | 235  |
| Conservation of energy . . . . .      | 134      | 'Epitome of Therapeutics,' by Stone   | 425  |
| Contagious ophthalmia . . . . .       | 429      | Erysipelas migrans, observations on   | 231  |
| Cooke's 'New Chemistry' . . . . .     | 157      | ——— treatment of . . . . .            | 234  |
| Cough, on the production of . . . .   | 493      | Eucalyptus globulus, researches on    | 502  |
| Cox's 'Introduction to Mental         |          | Eustachian tube, function of . . .    | 262  |
| Philosophy' . . . . .                 | 269      | Experience, surgical, by Lund . . .   | 352  |
| Croup of nasal mucous membrane        | 238      | Factory inspectors' reports (review)  | 107  |
| —— treatment of . . . . .             | 214      | Fayrer on poison of venomous snakes   | 248  |
| Cumming on antiseptic surgery . .     | 1        | Febrifuge properties of Eucalyptus    | 502  |
| Cunningham on use of Calabar          |          | Febrile disorders, quinine in . . .   | 509  |
| bean in tetanus . . . . .             | 510      | Fehling on rupture of vagina . . .    | 513  |
| Curtis on treatment of stricture      | 176, 357 | Fenger on hydronephrosis . . . .      | 213  |
| Cyon on innervation of uterus . . .   | 513      | —— case of stenosis of pul-           |      |
| Cyr on alimentation . . . . .         | 306      | monary artery . . . . .               | 216  |
| Dallinger on life history of monads   | 265      | Ferrand's 'Vade Mecum of Phar-        |      |
| Dalton on bile-spectrum . . . . .     | 259      | macy' . . . . .                       | 175  |
| Death from suffocation, signs of .    | 250      | Fever, lectures on, by Stokes . . .   | 367  |
| Delirium of persecution, by De Saulle | 415      | —— typhoid, its nature, &c., by       |      |
| Demarquay on regeneration of or-      |          | Budd . . . . .                        | 367  |
| gans and tissues . . . . .            | 343      | —— continued, treatise on, by         |      |
| Detection of blood-stains . . . . .   | 249      | Murchison . . . . .                   | 367  |
| Dickson, 'Lectures on Medicine in     |          | —— recent works on (review) . .       | 367  |
| relation to Mind,' &c. . . . .        | 407      | —— outbreak at Trondhjem . . .        | 220  |



|  | PAGE     |  | PAGE |
|--|----------|--|------|
| Fever treated by hydropathy . . . . .                        | 328      | Humphrey and Turner's 'Journal of Anatomy,' &c. . . . .              | 166  |
| ——— urinary, relations to am-<br>monia . . . . .             | 498      | Hydronephrosis, Fenger on . . . . .                                  | 213  |
| Fiorani's method of treating orchitis . . . . .              | 234      | Hydropathy applied to acute dis-<br>eases, by Armitage . . . . .     | 328  |
| Fleming on rabies and hydrophobia . . . . .                  | 49       | Hydrophobia, pathology of . . . . .                                  | 49   |
| ——— on transmissibility of<br>tuberculosis . . . . .         | 461      | Hygiene of schools, by Budgett . . . . .                             | 185  |
| Food and dietetics, Pavy on . . . . .                        | 306      | ——— report on, by Richardson . . . . .                               | 251  |
| ——— review of works on . . . . .                             | 306      | Immermann on progressive an-<br>æmia perniciosa . . . . .            | 487  |
| Foods, E. Smith on . . . . .                                 | 306      | Infection, parasitic fungi of . . . . .                              | 225  |
| Foot, surgery of, by Hancock . . . . .                       | 420      | Inflammation, pathology of . . . . .                                 | 128  |
| Forensic medicine, report on . . . . .                       | 242      | Inhalation of balsamic substances . . . . .                          | 510  |
| France, epidemics of cholera in . . . . .                    | 193      | Injections, use of ipecacuanha in . . . . .                          | 501  |
| French surgery, modern (review) . . . . .                    | 138      | Innervation of uterus . . . . .                                      | 513  |
| Friedreich on chronic hæmor-<br>rhagic peritonitis . . . . . | 496      | Insanity, hereditary . . . . .                                       | 414  |
| ——— on progressive muscular<br>atrophy . . . . .             | 414      | Ipecacuanha, use of, in injections . . . . .                         | 501  |
| Galloping consumption, relations of . . . . .                | 490      | Iverson on treatment of loose<br>bodies in joints . . . . .          | 215  |
| Gangrene, symmetrical, case of . . . . .                     | 222      | Jaccoud and Hallopeau on cerebral<br>congestion . . . . .            | 87   |
| Gardner on longevity . . . . .                               | 419      | Jackson, Hughlings, on cerebral<br>hæmorrhage . . . . .              | 87   |
| Garrod's 'Essentials of Materia<br>Medica' . . . . .         | 424      | James, Prosser, 'Lessons in Laryn-<br>goscapy' . . . . .             | 405  |
| Gastric secretion, causes affecting . . . . .                | 259      | Joints, loose bodies in, treatment of . . . . .                      | 215  |
| Gayton on smallpox . . . . .                                 | 292      | 'Journal of Anatomy and Phy-<br>siology' . . . . .                   | 166  |
| Genital canal, exudations around . . . . .                   | 516      | Kaczorowski on treatment of ery-<br>sipelas . . . . .                | 234  |
| Genito-urinary diseases, on, by<br>Van Buren . . . . .       | 357      | Kehrer on plastic surgery of nipple . . . . .                        | 236  |
| Gold Coast, life on the . . . . .                            | 392      | Kesteven on miliary sclerosis . . . . .                              | 187  |
| Golz on cold-water treatment of<br>fever . . . . .           | 329      | Ketli on diphtheric paralysis . . . . .                              | 524  |
| Gonorrhœa, use of endoscope in<br>treatment of . . . . .     | 217      | Key on structure of nervous system . . . . .                         | 260  |
| Gordon on the Gold Coast . . . . .                           | 392      | Kézmározy on alterations of<br>weight of new-born children . . . . . | 522  |
| Gosselin on ammoniacal urine . . . . .                       | 498      | Kleinwachter on pelvis in double<br>dislocation of hip . . . . .     | 521  |
| Gouley on stricture of the urethra . . . . .                 | 177, 357 | Kohts on production of cough . . . . .                               | 493  |
| Granular ophthalmia, pathology of . . . . .                  | 429      | 'Laboratory Guide,' by Church . . . . .                              | 158  |
| Grimshaw on smallpox epidemic in<br>Dublin . . . . .         | 292      | Laborde on the Paris Insurrection . . . . .                          | 17   |
| Hæmatemesis from aneurism of<br>coronary artery . . . . .    | 226      | Labour with contracted pelvis . . . . .                              | 518  |
| Hæmatoma pericysticum, cases of . . . . .                    | 225, 230 | Laryngoscopy, lessons in, by James . . . . .                         | 405  |
| Hæmorrhoids, treatment of . . . . .                          | 236      | Larynx, functions of nerves of . . . . .                             | 262  |
| Hæmorrhagic peritonitis . . . . .                            | 496      | Latschenberger on structure of<br>pancreas . . . . .                 | 255  |
| Hairs passed from bladder . . . . .                          | 234      | Laycock, disorders of memory . . . . .                               | 269  |
| Hamilton on after-treatment of<br>amputations . . . . .      | 1        | Lazarus on neuralgia of testis . . . . .                             | 236  |
| Hancock on surgery of foot . . . . .                         | 420      | Legrand de Saulle on delirium of<br>persecution . . . . .            | 415  |
| Harvey and his times . . . . .                               | 424      | ——— on hereditary<br>insanity . . . . .                              | 414  |
| Hawkins, Cæsar, collected works of . . . . .                 | 348      | Leopold on solid tumours of ovary . . . . .                          | 514  |
| Health, hints for, by Stocker . . . . .                      | 426      | Leprosy, Liveing on . . . . .  | 416  |
| Heart disease, lecture on . . . . .                          | 425      | Letheby on disinfectants . . . . .                                   | 252  |
| ——— temperature of . . . . .                                 | 255      | Létiévant on nerve sections . . . . .                                | 181  |
| ——— overstrain of . . . . .                                  | 485      | Leube on treatment of fever . . . . .                                | 328  |
| Hereditary insanity . . . . .                                | 414      | Lewes, problems of life and mind . . . . .                           | 269  |
| Hewson on the use of earth in surgery . . . . .              | 1        | Liebermeister on treatment of fever . . . . .                        | 328  |
| Hinton, questions of aural surgery . . . . .                 | 397      | Life assurance, Sieveking on . . . . .                               | 163  |
| Histology, report on, by Power . . . . .                     | 254      | Lime phosphate in effervescent<br>water . . . . .                    | 504  |
| Hooping-cough, quinine in . . . . .                          | 509      |  |      |
| Hospitals of Sweden, reports of . . . . .                    | 219      |  |      |

|                                      | PAGE     |  | PAGE |
|--------------------------------------|----------|--|------|
| Lime phosphate, use of, in phthisis  | 503      | Nerves of salivary glands              | 212  |
| Living on leprosy                    | 410      | Nerve sections, on, by Létiévant       | 181  |
| Liver, glycogenous function of       | 256      | Nervous diseases, use of nux vomica    |      |
| Longevity, Gardner on                | 419      | in                                     | 507  |
| Loven on lymphatics of stomach       | 215      | ——— system, structure of               | 260  |
| Luchsinger on glycogen in fever      | 256      | Nettleship on granular disease of      |      |
| 'Lunacy, Manual of,' by Winslow      | 162      | conjunctiva                            | 429  |
| Lund's surgical experience           | 352      | Neuralgia of testis                    | 236  |
| Lund on treatment of wounds          | 1        | Niemeyer on hyperæmia of brain         | 87   |
| Lungs, temperature of                | 255      | Nipple, plastic surgery of             | 236  |
| Lunier on mental alienation          | 17       | Noel, physical basis of mental life    | 269  |
| Lymphatic sheaths of choroid vessels | 255      | Nux vomica, uses of, in large doses    | 507  |
| Lymphatics of retina                 | 254      | Obstetrics, report on, by Wiltshire    | 513  |
| ——— of stomach                       | 215      | Obstetrical Society of London,         |      |
| Lymph-canalicular system             | 254      | 'Transactions' of                      | 171  |
| Man, triune nature of                | 269      | ——— of Dublin, 'Pro-                   |      |
| Mandelstamm on the decussation       |          | ceedings' of                           | 169  |
| of optic nerves                      | 263      | Oeffinger on uses of Eucalyptus        | 502  |
| Marshall on brain of a Bushwoman     | 81       | Esophagitis, diffuse, case of          | 224  |
| Massage, application of              | 216, 220 | Olshausen on incision of cervix uteri  | 514  |
| Materia medica, report on, by Semple | 504      | Ophthalmia, contagious, on             | 429  |
| ——— Pereira's                        | 420      | Optic nerves, decussation of           | 263  |
| ——— syllabus of                      | 420      | Oration on Harvey, by West             | 424  |
| ——— works on                         | 420      | Orchitis, treatment of                 | 234  |
| Maudsley on body and mind            | 269      | Orenellas, d', on action of emetine    | 247  |
| ——— on responsibility in men-        |          | Ovariectomy, case of, in a child       | 516  |
| tal disease                          | 269      | ——— statistics of                      | 226  |
| Meat extract, composition of         | 253      | Ovary, solid tumours of                | 514  |
| Medicine, report on, by Arlidge      | 487      | Overstrain of heart, features of       | 495  |
| ——— practical, by Silver             | 406      | Pædiatrics, report on                  | 522  |
| ——— mental, by Dickson               | 407      | Page on signs of death from suffo-     |      |
| ——— Scandinavian, report on,         |          | cation                                 | 250  |
| by Moore                             | 210      | Panas on use of cautery in prolapse    |      |
| Meigs and Pepper on diseases of      |          | of rectum                              | 523  |
| children                             | 172      | Pancreas, structure of                 | 256  |
| Memory, disorders and defects of     | 269      | Parasitic fungi of infection           | 225  |
| Mental medicine, lectures on, by     |          | Parkin on epidemiology                 | 292  |
| Dickson                              | 407      | Paterson's 'Life of Syme'              | 146  |
| Mercurial iodides, uses of           | 506      | Pathological Society's 'Transac-       |      |
| Merkel on development of sperma-     |          | tions'                                 | 170  |
| tozoids                              | 266      | Pathology, report on, by Arlidge       | 487  |
| ——— on hydropathy in fever           | 328      | ——— of apoplexy                        | 87   |
| Michel on chiasma of optic nerves    | 263      | ——— of fever                           | 367  |
| ——— on hydropathy in fever           | 329      | ——— of hydrophobia                     | 49   |
| Microcephalism, cases of             | 81       | Pavy on food and dietetics             | 306  |
| Midwifery, report on, by Wiltshire   | 513      | Pelvis, contracted, labour in          | 518  |
| Miliary sclerosis, by Kesteven       | 187      | ——— in double dislocation of hip       | 521  |
| Milk analysis, by Wanklyn            | 154      | Peritonitis, hæmorrhagic               | 496  |
| Mind and body, by Bain               | 269      | Pfleger on erysipelas migrans          | 231  |
| Monads, life-history of              | 265      | 'Pharmacy, Ferraud's Vade Mecum        |      |
| Moore's report on Scandinavian       |          | of'                                    | 175  |
| medicine                             | 210      | ——— practical, by Proctor              | 400  |
| Morano on lymphatic sheaths of       |          | Philadelphia Pathological Society,     |      |
| choroid vessels                      | 255      | 'Proceedings' of                       | 169  |
| Morrighia on urine and sweat         | 258      | Phosphate of lime, use of, in          |      |
| Müller, influence of skin on urinary |          | phthisis                               | 503  |
| secretion                            | 258      | Phthisis, use of raw meat in           | 503  |
| Murchison, treatise on fevers        | 367      | ——— temperature in                     | 174  |
| Muscular atrophy, Friedreich on      | 414      | Physical basis of mental life, by Noel | 269  |
| Nature of man, by Woodward           | 269      | 'Physiology, Mental, Carpenter's       |      |
| Nerves, decussation of optic         | 263      | Principles of'                         | 269  |
| ——— of larynx, functions of          | 262      | ——— report on                          | 254  |



|  | PAGE |  | PAGE |
|--|------|--|------|
| Pneumonia in children, its signs .                   | 223  | Sacrum, according to sex and age                           | 521  |
| Poison of Indian venomous snakes                     | 248  | Saffron, therapeutical uses of .                           | 500  |
| 'Problems of Life and Mind,' Lewes'                  | 269  | Salivary glands, nerves of .                               | 212  |
| 'Proceedings of Dublin Obstetrical Society' .        | 169  | Salomon on formation of glycogen in the liver .            | 256  |
| 'Proceedings of Philadelphia Pathological Society' . | 169  | Sansom on the antiseptic system .                          | 1    |
| Proctor's 'Practical Pharmacy' .                     | 400  | Saulle, Le Grand de, on hereditary madness .               | 414  |
| Progressive muscular atrophy .                       | 414  | ----- on the delirium of persecution .                     | 415  |
| ----- anæmia pernicioza .                            | 487  | Savignac on properties of saffron .                        | 500  |
| Prolapse of umbilical cord .                         | 519  | Scandinavian medicine, report on .                         | 210  |
| ----- of rectum, use of cautery in                   | 523  | Scepticism, psychology of .                                | 412  |
| Psychological doctrines, review of                   | 269  | Schech on functions of laryngeal nerves .                  | 262  |
| ----- medicine .                                     | 115  | Schiff on cardiac nerves .                                 | 264  |
| Psychology of communism .                            | 17   | ----- on inflammation .                                    | 128  |
| ----- of scepticism .                                | 412  | Schlesinger on thermometry of uterus .                     | 517  |
| Puerperal inflammations, statistics                  | 214  | Schmidt on prodromal stage of chorea .                     | 523  |
| ----- diseases, review on .                          | 318  | Scholz on cold-water treatment of fever .                  | 329  |
| ----- the, by F. Barker                              | 318  | Schools, hygiene of .                                      | 185  |
| ----- pyæmia, Duncan on .                            | 518  | Schorlemmer's 'Chemistry of Carbon Compounds' .            | 156  |
| Pustule, malignant, cases of .                       | 221  | Schuller on croup of nasal mucous membrane .               | 238  |
| Pyæmia, puerperal, Duncan on .                       | 518  | Schwalbe on lymphatics of the retina                       | 254  |
| Qualitative analysis, by Thorpe .                    | 159  | Sclerosis, miliary, Kesteven on .                          | 187  |
| Quarantine and cholera (review) .                    | 30   | Seitz on overstrain of heart .                             | 495  |
| ----- on Gulf Coasts of United States .              | 30   | Sieveking on life assurance .                              | 163  |
| ----- regulations .                                  | 30   | Signs of death from suffocation .                          | 250  |
| Quinine as an abortifaciant .                        | 517  | Silver's (A.) 'Practical Medicine' .                       | 406  |
| ----- uses of, in children's diseases                | 509  | Skin, Cauty on diseases of .                               | 179  |
| Rabies, Fleming on (review) .                        | 49   | ----- grafting, Caselli on .                               | 121  |
| Rapmund on quinine in children's diseases .          | 509  | ----- Woodman on .   | 121  |
| Raw meat, use of, in phthisis .                      | 503  | ----- its influence on secretion of urine                  | 258  |
| Reeves' report on surgery .                          | 230  | Smallpox epidemic, Dublin, report on, by Grimshaw .        | 292  |
| Regeneration of organs and tissues                   | 343  | ----- prevention of, by Douglas                            | 292  |
| Reports of factory inspectors .                      | 107  | ----- notes on, by Gayton .                                | 292  |
| Report on materia medica, by Semple                  | 500  | ----- pathology of, Bakewell on                            | 292  |
| ----- medicine, by Arlidge .                         | 487  | ----- predisposing conditions and preventives, by Both .   | 292  |
| ----- midwifery, by Wiltshire                        | 513  | ----- statistics at Stockholm                              | 229  |
| ----- physiology, by Power .                         | 254  | ----- on, by Colin .                                       | 292  |
| ----- Scandinavian medicine, by Moore .              | 210  | Smith, Andrew, on the caisson disease .                    | 253  |
| ----- surgery, by Reeves .                           | 230  | Smith, Edward, on foods .                                  | 306  |
| ----- toxicology, by Richardson                      | 242  | Smyth, Woods, on Bible and evolution .                     | 167  |
| Responsibility in mental disease .                   | 269  | Snakes, poison of venomous .                               | 248  |
| ----- legal, in old age .                            | 411  | Spectrum of bile .   | 259  |
| Retina, connections with brain .                     | 264  | Speech, tongue not essential to .                          | 427  |
| Retroflexion of uterus with pregnancy .              | 224  | Spermatozoids, development of .                            | 266  |
| Retzius on structure of nervous system .             | 260  | Spiegelberg on exudations around pelvic organs in female . | 516  |
| Reynolds and Bastian on congestion of brain .        | 87   | ----- on labour in contracted pelvis .                     | 518  |
| Riegel on the cold-water treatment of fever .        | 329  | ----- on primary cancer of uterus .                        | 514  |
| Ringer on temperature in phthisis                    | 174  |  |      |
| Rizzoli on papilloma of umbilicus .                  | 235  |  |      |
| Robin on ammoniacal urine .                          | 498  |  |      |
| Rodwell's 'Birth of Chemistry' .                     | 156  |  |      |
| Roosa on diseases of the ear .                       | 394  |  |      |
| Rossander on infantile cataract .                    | 212  |  |      |
| Rumbold on Eustachian tube .                         | 261  |  |      |

|   | PAGE |   | PAGE     |
|---|------|---|----------|
| Spinal cord, tuberculosis of . . . . .                          | 489  | 'Transactions of Pathological Society' . . . . .                      | 170      |
| Stefani on nux vomica in nervous diseases . . . . .             | 507  | Transmissibility of tuberculosis . . . . .                            | 461      |
| Steffanini on subcutaneous use of calomel . . . . .             | 505  | Traumatic tetanus treated by Calabar bean . . . . .                   | 510      |
| Stethoscopic signs of pleuritis in children . . . . .           | 223  | Treatment, modern, of stricture (review) . . . . .                    | 357      |
| Stewart on conservation of energy . . . . .                     | 134  | Tubercle, identity of, in man and the lower animals . . . . .         | 462      |
| Stokes' 'Lectures on Fever' . . . . .                           | 367  | Tuberculosis, acute, connection with galloping phthisis . . . . .     | 490      |
| Stocker's 'Hints for Health' . . . . .                          | 426  | ———— of spinal cord . . . . .   | 489      |
| Stockholm, statistics of smallpox . . . . .                     | 229  | ———— transmissibility of, on, by Fleming . . . . .                    | 461      |
| Stone's 'Epitome of Therapeutics' . . . . .                     | 425  | Tuke and Bucknill's 'Psychological Medicine' . . . . .                | 115      |
| Stricker on temperature of heart and lungs . . . . .            | 255  | Tumours of ovary, solid, on . . . . .                                 | 514      |
| Stricture, cases of, by Teevan . . . . .                        | 357  | Twisleton on tongue not essential to speech . . . . .                 | 427      |
| ———— treatment of, Curtis on . . . . .                          | 176  | Umbilical cord, prolapse of . . . . .                                 | 520      |
| ———— modern treatment of (review) . . . . .                     | 357  | Umbilicus, papilloma of . . . . .                                     | 235      |
| Suffocation, death from, signs of . . . . .                     | 250  | Urinary fever and ammoniacal urine . . . . .                          | 498      |
| Surgery, antiseptic . . . . .                                   | 1    | ———— organs, diseases of, by Gouley . . . . .                         | 177, 357 |
| ———— aural, questions of, by Hinton . . . . .                   | 397  | ———— secretion influenced by skin . . . . .                           | 258      |
| ———— report on . . . . .  | 230  | ———— passages, diseases of, by Sir H. Thompson . . . . .              | 357      |
| ———— of foot, Hancock on . . . . .                              | 420  | Urine and sweat, relations between . . . . .                          | 258      |
| ———— of the war of the American Rebellion (review) . . . . .    | 65   | ———— blood in, detection of . . . . .                                 | 225      |
| ———— modern French (review) . . . . .                           | 138  | ———— ammoniacal, its effects . . . . .                                | 498      |
| Surgical emergencies, by Swain . . . . .                        | 402  | Uterine cervix, incision of, its uses . . . . .                       | 514      |
| ———— experience, by Lund . . . . .                              | 352  | Uterus, cancer of . . . . .   | 514      |
| ———— inquiries, by Jordan . . . . .                             | 184  | ———— innervation of . . . . .   | 513      |
| ———— operations, by Bell . . . . .                              | 176  | ———— retroflexion of, in conception and pregnancy . . . . .           | 224      |
| Swain on surgical emergencies . . . . .                         | 402  | ———— thermometry of . . . . .   | 517      |
| Sweat and urine, relations between . . . . .                    | 258  | Vagina, spontaneous rupture of . . . . .                              | 513      |
| Sweden, mortality of, in 1871 . . . . .                         | 220  | Valecourt on Cannes and its climate . . . . .                         | 168      |
| Syllabus of materia medica . . . . .                            | 424  | Van Buren on diseases of genito-urinary organs . . . . .              | 357      |
| Syme, life of, by Paterson . . . . .                            | 146  | Voillmier on hæmorrhoids . . . . .                                    | 236      |
| Syphilis, diagnosis of, by injection of calomel . . . . .       | 505  | Varicocele of lower extremities . . . . .                             | 237      |
| ———— transmitted by circumcision . . . . .                      | 251  | Wanklyn's milk analysis . . . . .                                     | 154      |
| Temperature in phthisis . . . . .                               | 174  | Water, use of, in disease (review) . . . . .                          | 328      |
| ———— of lungs and heart . . . . .                               | 255  | ———— in fever, by Merkel . . . . .                                    | 328      |
| Testicle, carcinomatous cystoma of . . . . .                    | 218  | ———— with phosphate of lime, use of . . . . .                         | 504      |
| Tetanus, use of chloral in . . . . .                            | 504  | Watson's new operation for ankylosis of elbow . . . . .               | 239      |
| ———— treated by Calabar bean . . . . .                          | 510  | Weight, on, of new-born children . . . . .                            | 522      |
| Therapeutics of mercurial iodides . . . . .                     | 500  | Wells, case of ovariectomy in a child . . . . .                       | 516      |
| ———— of alcohol . . . . .                                       | 511  | West's 'Diseases of Infancy' . . . . .                                | 159      |
| ———— report on, by Semple . . . . .                             | 500  | ———— oration on Harvey . . . . .                                      | 424      |
| Thermometry of uterus . . . . .                                 | 517  | 'What am I? Introduction to Mental Philosophy,' &c., by Cox . . . . . | 269      |
| Thompson's Conspectus, by Birkett . . . . .                     | 424  | Wilson's 'Anatomist's Vade Mecum' . . . . .                           | 167      |
| Thompson, Sir H., on diseases of the urinary passages . . . . . | 357  | ———— 'Guide to Zoology' . . . . .                                     | 150      |
| Thorpe's 'Qualitative Analysis' . . . . .                       | 159  | Wiltshire's report on obstetrics . . . . .                            | 513      |
| Thyroid, structure of . . . . .                                 | 258  | Winslow's 'Manual of Lunacy' . . . . .                                | 162      |
| Tissues, regeneration of . . . . .                              | 343  | Works, collected, of Cæsar Hawkins . . . . .                          | 348      |
| Tongue not essential to speech . . . . .                        | 427  | Zoology, guide to, by Wilson . . . . .                                | 150      |
| Toxicology, report on . . . . .                                 | 242  | Zymotics, on (review) . . . . .                                       | 292      |
| Toxicological action of emetine . . . . .                       | 247  |   |          |
| ———— properties of putrefying blood . . . . .                   | 242  |   |          |
| Transactions of Obstetrical Society . . . . .                   | 171  |   |          |



















